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F331
V. 10-11

AMERICAN JOURNAL of PHYSICS

(Formerly THE AMERICAN PHYSICS TEACHER)

A Journal Devoted to the Instructional and Cultural Aspects of Physical Science

VOLUME 10

FEBRUARY, 1942

NUMBER 1

The Time Concept in Restricted Relativity

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1. The theory of relativity had an immense educational influence on physicists and other scientists by teaching them the approach to scientific concepts that has been more recently named *the operational point of view*. According to it, a physical concept is not completely defined until an instrumental operation is described which permits of reducing the concept to precise measurements. In relativity the operational approach was systematically carried through in application to the notions of time and space which, in spite of their antiquity, were lacking in scientific precision. The process of converting these notions into scientific concepts, undertaken and accomplished by the theory of relativity, served to resolve some grave apparent contradictions between the results of equally accurate physical experiments. In particular, in relation to time, the operational approach is quite straightforward, as the theory simply insists that a moment of time is nothing but the reading of a clock and that time itself (comprising all time moments) is the totality of all possible clock readings.

It is the opinion of the present writer that any presentation of the theory of relativity intended for wider circles should particularly emphasize its operational aspect. Indeed, on the one hand, the main philosophic value of this theory is that it has clarified the concepts of space and time by divesting them of their former metaphysical connotations. On the other hand, this side of

the subject can be treated with a minimum of technical prerequisites and is completely within the grasp of the nonmathematical reader.

The occasion for the foregoing remarks is the appearance of a new popular booklet by Professor Herbert Dingle¹ which not only neglects to bring out the operational tendencies of relativity but flatly denies them by implication. As the contentions advanced in this little book are sure to cause confusion in the minds of the laymen and young students for whom it is intended, we propose to clear up the misapprehensions on which they are based in the following sections. They are meant for those readers who, having a very slight acquaintance with the theory of relativity, wish for a more detailed discussion of its fundamental concepts.

2. One of the consequences which the theory of relativity deduces from its fundamental assumptions is that the rate of a clock is changed when the latter is set in motion with constant velocity. According to the aforementioned operational view, this means that *the time scale is changed* for the observer moving with the clock and relying on it for his time measurements. In fact, time is nothing but all the successive positions of a clock, and these positions follow one another at different rates when the clock is at rest and when it is moving. Such a conclusion presupposes, of course, that the rate of every

¹ H. Dingle, *The special theory of relativity* (Chemical Publishing Co., New York, 1941).

correct clock is changed by motion in the same way; and this is precisely what the theory of relativity claims. According to it, any clock after being set in motion with the velocity v slows its rate down in the proportion

$$\kappa = \left(1 - \frac{v^2}{c^2}\right)^{\frac{1}{2}}, \quad (1)$$

where c is the speed of light. Therefore, time intervals are longer, when measured by the moving clock, in the inverse ratio $1/\kappa$. If a time interval measured by any resting clock is Δt , it is increased to

$$\Delta t' = \frac{1}{\kappa} \Delta t \quad (2)$$

when measured by the identical clock set in motion (with the velocity v). Equation (2) is known as the *law of time dilatation*.

The divergence of Dingle's views from those generally held sets in at this point. He describes several instruments² which, in his opinion, would permit of time measurement and, thus, would constitute clocks and which, nevertheless, upon being set in motion, change their rates not in the same way but differently. He claims to have established by this means a contradiction with the usual interpretation of relativity. As the whole argument rests on the devices proposed by him as timepieces, it will be necessary here to go into their construction.

The clocks considered by him are of the hourglass type: the elapsed time is measured by the amount of sand having run from an upper into a lower container. The quantity of sand in the lower container can be determined in several ways: (a) by measuring its volume, (b) by weighing it, (c) by counting the grains. In this way the hourglass principle gives rise to three different constructions which we shall differentiate by labeling them (a), (b) and (c).

As long as the three kinds of clocks are all at rest with respect to the observer, their scales can be adjusted so that they have the same rate and are perfectly equivalent because both the volume of the sand and its weight are proportional to the number of grains in it. It is otherwise when the clocks are given a velocity v with

² The description is given more fully than in the book in a paper of Dingle's [Nature 144, 888 (1939)].

respect to the observer. The volume and weight are still proportional to the grain number but the constants of proportionality are not the same as before. In fact, the theory of relativity shows that, in consequence of the motion, the volume of a given number of grains contracts in the proportion κ while their weight increases in the ratio $1/\kappa$. Therefore, the rates of the three moving clocks are not the same but stand in the proportions $\kappa : 1/\kappa : 1$, provided that their rates did not differ from one another when at rest.

It goes without saying that the construction of a clock may be faulty—in which case, properly speaking, it is not a timepiece at all—also that it is possible to use a perfectly good clock in a faulty manner. All we could conclude so far was that the three constructions of Dingle's may be used with impunity by an observer with respect to whom they are at rest. Whether they remain legitimate timepieces in the more general case when they are in motion, with respect to the observer, remains still to be seen. Therefore we shall discuss in the next section the *criterions that must be satisfied by a legitimate clock*.

3. We choose as our starting point the so-called Lorentz transformation, with respect to which there is no disagreement, as it is accepted as valid by Dingle. Let us suppose that two observers A and A' are provided with identical yard sticks and clocks and move, with respect to each other, in a direction x and with the relative velocity v . We shall denote by x, y, z, t the space coordinates and time reading, as measured by A , and by x', y', z', t' the same quantities, as measured by A' . The question whether Dingle's clocks (or any other constructions) are legitimate does not yet arise since each observer uses a clock resting with respect to himself whereas our doubts of legitimacy pertain only to moving clocks.

The results of the measurements of the two observers stand in a relation given by one of the most fundamental set of formulas in relativity known as the *Lorentz transformation*,

$$\begin{aligned} x' &= \frac{1}{\kappa} (x - vt), & t' &= \frac{1}{\kappa} (t - vx/c^2), \\ y' &= y, & z' &= z. \end{aligned} \quad (3)$$

We have only stated how the measurements must be effected—namely, by two sets of yardsticks and clocks—and have not yet specified what is to be measured. Equations (3) have a meaning only in the case where the quantities x, y, z, t and x', y', z', t' refer to an *event*, that is, to a happening which can be recognized independently of any system of coordinates. We shall give here a few examples of what is meant by *events* and from which the general characteristics of the latter may be inferred.

As a first example let us take an atom that is moving in space and suddenly emits a wave train of radiation. The observers A and A' are watching this single atom from their respective systems. Here the event is the beginning of the emission, and the coordinates x, y, z, t (or x', y', z', t') represent the position of the atom and the clock reading as measured by the observer A (or A') at the moment when the event takes place.

As a second example we may consider a marble rolling along a slat and passing over an ink line drawn across it. Again, the motion of the slat and of the marble will appear different to the two observers. The event for which they have to watch, in this case, is the passage of the marble over the ink mark. The coordinates x, y, z, t now represent the position of the marble and the clock reading at the moment when the marble and the ink line are conjoined.

The essence of the two examples is that the events are specified in a way independent of any particular system of coordinates. The two observers are rightly convinced that they are watching from their different angles the same phenomenon so that the two sets of measurements (x, y, z, t and x', y', z', t') which they obtain represent but different aspects of the same event (or point in space-time).

In the case of the succession of two events, both being watched and measured by the observers A and A' , Eqs. (3) can be applied to both. If the coordinates of these two events are respectively characterized by the subscripts 1 and 2, the subtraction of the first set of Eqs. (1) from the second gives

$$(3) \quad \begin{aligned} \Delta x' &= -(\Delta x - v\Delta t), & \Delta t' &= -(\Delta t - v\Delta x/c^2), \\ &\kappa && \kappa \\ \Delta y' &= \Delta y, & \Delta z' &= \Delta z, \end{aligned} \quad (4)$$

where Δx is $x_2 - x_1$, Δt is $t_2 - t_1$, and so forth—the intervals in space or time between the two events.

4. We have particularly insisted that the quantities Δx and $\Delta x'$, Δt and $\Delta t'$, and so forth, represent intervals between the same events measured from two different frames of reference. This is contrary to the customary exposition, which usually emphasizes the circumstance that the observers A and A' obtain different pictures of the phenomena. It must be admitted that the novelty and essential interest of the theory of relativity lies in this difference of aspects. Most relativists regard the fact that both observers are watching the same happenings as so self-evident that it does not need stressing. Indeed, what relations could there exist between their observational results if they were measuring different things? Nevertheless, Dingle's difficulties arise precisely from his failure to realize this fundamental requirement. Therefore, we repeat emphatically that the equations of the Lorentz transformation in the forms (3) and (4), as well as all other equations of the theory of relativity, apply only to the case when the primed and unprimed symbols in them refer to the same events, which must be defined independently of any frame of reference.

When this requirement is understood, the question of the legitimate clock construction becomes quite simple. It is identical with the question of the transformation of time units. If the time units defined by clocks are subject to the transformation expressed by Eq. (4), they cannot be anything but time intervals elapsed between two events, in the sense of the preceding section. Thus, every reading of a legitimate clock must be itself an event. This condition is satisfied by any dial clock since the fact that the clock hand points to the number 12, or to any other mark on the dial, is a statement quite independent of any reference system. It is also satisfied by any rotating or oscillating mechanism—the earth, a revolving atom, a pendulum—since the return to the original condition after a revolution or oscillation marks an interval between two events, specified in the same independent way. The same may be said of practically every clock construction that is in actual use. If the clock is resting with respect to

the observer A , the time interval $\Delta t [=t_2-t_1]$ between two specified positions is measured *in the same place* ($x_2=x_1$, $y_2=y_1$, $z_2=z_1$, or $\Delta x=\Delta y=\Delta z=0$), neglecting the insignificant displacements and velocities within the clock mechanism. On the other hand, the observer A' will measure as the interval between the two positions on the same clock (which has the velocity v with respect to him) the amount

$$\Delta t' = \frac{1}{\kappa} \Delta t,$$

as obtained from the second of Eqs. (4) when account is taken of the condition $\Delta x=0$.

Therefore, the time interval will appear to him longer in the proportion $1/\kappa$ in conformity with the Eq. (1) of time dilatation, which is thus seen to apply to any legitimate clock and (as explained in Sec. 2) to time itself.

5. Of Dingle's three contraptions of the hour-glass type, described in Sec. 2, only the construction (c) can be regarded as a clock, in the light of the preceding explanations. In fact, the falling of the n th grain is an event recognizable independently of any reference system. Another legitimate construction would be to make a mark on the lower vessel and to define as the time unit the time in which the vessel is filled with sand up to the mark. On the other hand, in the constructions (a) and (b), not only is the method of measurement individual with the observers (each measuring with his own instruments) but so also is the very interval which is measured. In fact, we have just recognized that the two observers measure the same interval only if they count the same number of falling grains. However, Dingle himself admits that, in the cases (a) and (b), the grain number per unit time (as defined by him) falling into the lower container is not the same in motion and at rest. It is, therefore, certain that the two observers do not measure intervals between the same events, and it is impossible to see what significance their measurements can have either for relativity or for any other scientific use. The situation may be compared with the geometric problem of the projection of a straight segment on Cartesian axes (Fig. 1), of which problem the Lorentz transformation is, indeed, a generalization. Suppose we have a rigid rod with two

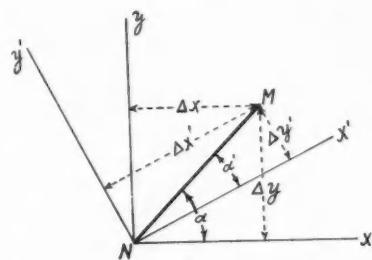


FIG. 1. Projection of a straight segment on Cartesian axes.

notches M and N , the distance MN between them being denoted by Δl . Let the observer B measure the distances Δx and Δy between the projections of the notches on the x - and y -axes and the observer B' the analogous distances $\Delta x'$, $\Delta y'$ in the projection on the x' - and y' -axes. According to the Pythagorean theorem there exists the relation $(\Delta x^2 + \Delta y^2) = (\Delta x'^2 + \Delta y'^2) = \Delta l^2$, which is contingent, however, upon the condition that the observers B and B' measure distances between projections of *the same two points* M and N . On the other hand, the observers B and B' could proceed differently: they might determine the projections Δx , $\Delta x'$ as before, but measure in the y - and y' -directions, not at all the projections of the points M , N but some distances depending on the angles of inclination α and α' , for instance, $f(\alpha)$ and $f(\alpha')$, respectively, where $f(\alpha)$ is some arbitrarily chosen function of the angle. Such a procedure cannot be said to have much purpose, as it is ill designed to bring out the invariant length of the distance Δl ; but it is the exact analog of the scheme proposed by Dingle for the relativistic observers A and A' in his clock constructions (a) and (b). In these cases the efforts of the observers would have hardly more relation to time measurement than the famous attempt of Sir Isaac Newton, who, wishing to boil a four-minute egg, reputedly, put his watch into the boiling water and was found by his housekeeper staring at the egg in his hand.

6. It will be well to discuss another point in Dingle's book which is sure to be confusing to the reader, his repeated insistence that the relativistic contraction of solid bodies is *not real* but due only to "the mental attitude of the observer." This phrase, presumably, refers to the fact, brought out by relativity, that there is

something invariant behind the several aspects in which the same thing appears to different observers. Only that invariant thing in itself is regarded by him as real, in this sense, and not its measurements by the several observers, although their "mental attitude" is here of less account than their state of motion.

We think, however, that such a transcendental use of the word *real* is misleading for the beginner as it tends to obscure the fact that the theory of relativity provides *for every observer and frame of reference* a complete and self-consistent world picture, including all dynamical and causal relations of the observations. If his measurements show to the physicist that a rod, on being set in motion, shortens or that a moving clock begins to go slower, these changes are just as real to him as any other empiric facts, because the ultimate reality of the physicist lies in the observational data collected by him. He may legitimately ask *why these changes occur*, and he can give a satisfactory answer by following through the interplay of forces which leads to these phenomena.

In fact, this question was asked and answered long before the theory of relativity was discovered. If it is assumed that a solid body consists of electrified particles and that the forces holding them together are of electric nature, these forces of interaction must undergo changes when the solid is set in motion. Indeed, each moving electric charge produces a magnetic field in which the other electrified particles move, thus experiencing also magnetic forces. This problem was treated by Morton and Searle in 1896, who found the following law of transformation: if an electrostatic force in a resting system has the components F_x , F_y , F_z , it becomes in the moving system an electrodynamic force

$$F'_x = \kappa^2 F_x, \quad F'_y = \kappa F_y, \quad F'_z = \kappa F_z. \quad (5)$$

As before, the x -axis is chosen in the direction of the uniform motion. It is clear that a configuration of atoms which is in equilibrium in the state of rest will no longer be so upon being set in uniform motion but will tend to rearrange itself in a new equilibrium. It was shown by H. A. Lorentz in the years 1896 and 1905 that the new equilibrium configuration of solid bodies corresponds exactly to the Lorentz contraction.

This result was obtained on the basis of the classical theory; in the theory of relativity it was shown that Eqs. (5) express the general law for the transformation of any forces, the unprimed components being those measured by the resting observer (A of Sec. 3), the primed ones by the moving observer (A'). This means that Lorentz's result is true also in the case when the interatomic forces are partly or wholly of nonelectromagnetic nature. At any event, the physicist possesses a complete dynamical explanation of the mechanism of the Lorentz contraction, which is thus *real* in the ordinary sense of the word.

The same may be said with respect to the time dilatation. It was shown in Sec. 4 that every legitimate clock slows down in the ratio $1/\kappa$ upon being set in motion. However, in every case there is a dynamical cause for this effect. We shall follow this through for the pendulum clock. Let us take the simplest kind of pendulum, consisting of a rigid rod of negligible weight suspended at its upper end and with a massive particle attached to its lower end. Let the pendulum with its support be set in motion in the x -direction with velocity v , and let the plane of oscillations be turned so that the oscillations of the bob cut the x -direction at a right angle (from the point of view of the observer moving with the pendulum). In this case, the kinetic energy of the oscillations can be readily separated from the kinetic energy of the translatory motion and the usual theory of the pendulum can be applied. For oscillations of small amplitude this leads to the well-known formula for the period,

$$T' = 2\pi(m'l'/F')^{\frac{1}{2}}, \quad (6)$$

where l' denotes the length of the pendulum, m' is its mass and F' is the force acting on this mass (the normal direction of the pendulum rod coinciding with the direction of the force).

In the particular case of the resting support of the pendulum ($v=0$) we use unprimed letters,

$$T = 2\pi(ml/F)^{\frac{1}{2}}. \quad (7)$$

The relation between T' and T can be obtained from Eqs. (7) and (8) if the transformation formulas for m , l and F are known. As to the mass m , both the theory of relativity and Lorentz's classical theory of the electromagnetic mass give $m' = m/\kappa$.

With respect to l and F , we have to distinguish two cases:

(1) The direction of the pendulum rod (in equilibrium) is *transverse*, that is, normal to the x -direction determined by the translatory velocity v . If we call this the y -direction ($l=l_y$, $F=F_y$), we find from Eqs. (4) and (5) that $l'=l$ and $F'=\kappa F$.

(2) The direction of the pendulum rod is *longitudinal*, that is, it coincides with the x -direction ($l=l_x$, $F=F_x$). In this case the

Lorentz contraction gives $l'=\kappa l$ and Eqs. (5), $F'=\kappa^2 F$.

In either case we obtain by substituting into Eqs. (6) and (7), $T'=T/\kappa$, the now familiar Eq. (2) of the time dilatation. In the case of a pendulum clock the change of the time scale is the result of a dynamical process that is clear to us in all its details, and it is possible to carry through a similar analysis for other clock constructions. Therefore, it is hardly expedient to call the time dilatation an *apparent* phenomenon.

Is the Frequency Theory of Probability Adequate for All Scientific Purposes?

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WAS it not Carlyle or perhaps Ruskin who took offense at the terms "objective" and "subjective"? He urged, as I recollect, that instead of saying "It is objectively so," we should say "It *is* so," and instead of saying "It is subjectively so," we should say "It *seems* so." The low regard for the subjective implicit in this dictum is prevalent in scientific circles, and rightly so, since objectivity is the prime characteristic of scientific thought. Nevertheless, it is possible for the objective ideal to lead to excesses. In our admiration for the self-effacement of the work of the cold scientific mind we may unrealistically disregard at times the basic fact that subjective motives are the springs of human conduct. In the study of scientific methodology we may overlook the fact that the very conception of an objective external world is the creation of primary subjective experience.

It seems to me that this excessive regard for the objective is at the root of a serious defect in the argument for the exclusive use of the frequency-in-a-collective interpretation of the probability concept in scientific work. A major premise in the argument—well stated by Professor Bergmann in a recent issue of this journal¹—is that science is concerned only with statements that can be put to experimental test, that is, with "objective" statements. The notion of probability as a measure of expectation and, in fact, every sort of single-event probability is

swept aside as unscientific and "popular" because probabilities of this type are not subject to experimental test. Plausible as this doctrine is, it involves a somewhat obvious danger since the concern of science with objective law can hardly justify complete disregard of its roots in subjective experience. Moreover, there is at least one place in scientific theory where the probability of single events plays an important part. I refer to certain basic theorems of statistical mechanics.

To this question of the place of the different interpretations of probability needed in statistical mechanics I will return. First, however, I must clear the ground by developing the thesis of this paper from another point of view. This thesis is simply that it is possible and helpful to define and use single-event probability in spite of the fact that this kind of probability cannot be tested by experiment and is not wholly objective.² Complete rejection of the frequency-in-a-collective interpretation is not implied. In my opinion there are two basic varieties of probability, each applicable in its own domain. The frequency interpretation is universally valid for one, but not for the other.

Single-event probability can be described as a quantified expression of rational judgment rela-

¹ G. Bergmann, Am. J. Phys. 9, 263 (1941).
² Single-event probability is ordinarily grounded in direct subjective experience. It may be said to belong to the subjective world of experience. On the other hand, it has been so developed as to claim, so far as possible, that universal consent of rational men which we associate with objectivity.

tive to primitive from the elsewhere process second the e proba since object but a exten like a of pro someti cision like e wholly This i quenc Recog exper diffic fact t series bility the li Wh possi been estab indivi expre prima accep variet exten Henc distin emph been with with study

³ E. ⁴ In the te nation ever, comm to rep

tive to given information or data. It is the primitive form of the concept arising directly from the experience of expectancy. As such I have elsewhere³ called it *primary probability*. By a process of extrapolation we have developed a second probability concept which we "locate" in the external world of nature. This type of probability may be described as "objective," since we think of it as a characteristic of an objective world. It is not relative to information, but absolute. On the other hand, since the external world is an interpretation of experience, like any theory in physics, this objective variety of probability is in a sense "theoretical."⁴ It is not something we can compute with complete precision directly from the data of experience but like every "law of nature" is always a never-wholly-verified extrapolation from experience. This is the type of probability to which the frequency interpretation is primarily applicable. Recognition of its status as an extrapolation from experience does away at a blow with the primary difficulty in the frequency theory, namely, the fact that our data never give more than a finite series of trials, whereas the definition of probability as a frequency involves the evaluation of the limit of an *infinite* series.

When the observation of the occurrence of a possible event K in a series of similar trials has been sufficiently extended to give rise to a well-established objective law of probability, the individual confronting an additional trial will express his expectancy of K by means of a primary probability numerically equal to the accepted objective probability. Thus the two varieties are closely related. They are to a large extent subject to the rules of the same calculus. Hence it is not altogether surprising that the distinction between them has not been generally emphasized. A chief cause of the oversight has been the preoccupation of workers in the field with the accepted mathematical calculus and with the axiomatic methods in vogue in the study of the logical basis of mathematics.

³ E. C. Kemble, Phil. Sci. 8, 204-232 (1941).

⁴ In my previous essay on this subject (reference 3) I used the term "theoretical probability" as the standard designation of probability located in the external world. However, this nomenclature does not conform well to the common usage of the word "theoretical," and it seems wise to replace the above phrase by "objective probability."

THE PROBABILITY OF SINGLE EVENTS

Everyone has a feeling for the probability of single events, but the formulation of a satisfactory definition of this primary type of probability has caused difficulty.⁵ Recognizing the subjective element in the concept, Ramsay has identified it with the measure of partial belief. Keynes, on the other hand, emphasizing our desire to separate the rational basis of expectation from the matrix of individual feeling, calls probability an "objective logical relation between propositions," that is, between premise, or experimental trial, on the one hand and conclusion, or experimental result, on the other. Neither of these authors satisfies the demand of the present-day physicist for a definition that fixes the method of arriving at numerical values. Each of the definitions has to be implemented by a series of *a priori* axioms⁶ which lead to a rule for the evaluation of numerical values. The reader may not share the author's suspicion of such axioms, but most physicists will no doubt prefer to dispense with them if possible.

These difficulties can be largely overcome if we distinguish between what we are trying to do in the evaluation of probabilities and what we actually succeed in doing. To understand our purpose will require a little amateur psychologizing.

We all know that when we are confronted with the possibility of an event whose occurrence or nonoccurrence is of importance or interest to us, we have a feeling of expectancy which is a part of our mental adjustment to the situation. In its most primitive form this sense of expectancy comes to a young child as a result of the repetition of a relatively stable pattern of experience. It is a psychological fact that, as a result of such repetition, the child becomes educated, so that after experiencing a portion of the pattern he adjusts himself to the remainder. This adjustment is no doubt relatively simple in the case of a month-old baby, but becomes more and more complex as the child passes from infancy to maturity. Mental images of the complete pattern and of possible variations from its normal form

⁵ Some of the difficulties are indicated by Bergmann, reference 1. For a fuller review the reader is referred to C. E. Bures, Phil. Sci. 5, 1-20 (1938).

⁶ Jeffreys characterizes these axioms as "*a priori* propositions, accepted independently of experience, and making by themselves no statement about experience." Cf. H. Jeffreys, *Theory of probability* (Oxford, 1939), p. 8.

present themselves. In these images lies the basis of thought. Frequently the child becomes acquainted with two or more patterns which start out in the same manner. After experiencing the common introduction to such a set of patterns the child must adjust itself to two or more possibilities. The adjustment will vary with the relative frequency with which different members of the set have been experienced in the past. An analysis of the developing complexity of the reaction with growing experience is not necessary here and would be in any case beyond my ability. Suffice it to note that probability calculations are part of our attempt to make appropriate preparation for the uncertain future. The primitive feeling of expectancy, or sense of likelihood, is a part of the adjustment originating in the statistics of similar past experiences. In maturity, however, this feeling is fed by elements that are not of a directly statistical nature. The growing child develops the sense of certainty regarding invariable rules of experience. The notion of the external world, abstract ideas, in fact the whole paraphernalia of adult thought and philosophy, come as echoes to experience whose reverberations make an ever-present contribution in our adjustment to the momentary situation. These things affect our sense of expectancy. We anticipate strongly events in accord with accepted theory and often reject the direct evidence of our senses if it is in too gross conflict with theory.

Chief among the fruits of maturity are the acceptance of the external world and faith in the possibility of objective knowledge. The external world is an abstraction from experience characterized by rules of invariance and by the fact that it is shared by other people. Whatever pertains to the external world or has the properties we associate with that world we call "objective." We find that it pays to adjust our behavior to objective fact insofar as objective fact is attainable, and we have developed the concept of probability in the endeavor to establish an objective numerical measure to supplement and *correct* our intuitive sense of expectancy. Some measure of success has been achieved, but it is of the first importance to recognize that complete success is wholly impossible.

I accordingly suggest that we define a single-

event probability as a number computed or estimated from available information and giving, so far as possible, an objective correlate to our sense of expectancy with respect to an event, or belief in a statement of fact. To insure maximum objectivity the probability should be computed, whenever possible, in accordance with an accepted rule. *The rule then becomes part of the complete definition.* It was my initial notion that we should limit the term "probability" to those cases in which such a definite rule of computation is applicable, but further consideration convinces me that it is futile to introduce such a limitation. The procedure for the evaluation of the appropriate numerical measure of expectancy cannot be completely standardized except for exceptional standardized situations and hence an arbitrary element enters into the formation of the judgment. This element is prominent in some cases, trivial in others. Methods of minimizing the arbitrary factor and increasing the objectivity of probability judgments deserve much more study than I have been able to give, but they can never completely gain the end desired.

The relation between probabilities, on the one hand, and expectancy, or belief, on the other, bears a similarity to the relation between thermometer readings and our sense of heat and cold.³ However, the technique of thermometer design has been far more successful in making possible unique and objective temperature measurements than the technique of evaluating probabilities in carrying out its corresponding task.

The simplest and, without doubt, the most fundamental variety of situations for which a satisfactory fixed rule for computing probabilities exists is undoubtedly that in which we are confronted with an experiment or trial which has been carried out many times in the past. Past experience forms the only evidence on which we can possibly judge the future, and it is in the long series of similar trials that experience takes its simplest and most convincing form. Our confidence in the outcome of the next trial goes hand-in-hand with the percentage of successes in the past—at least in all cases where the direct past trials are the only source, or the strikingly preponderant source, of available evidence. In this situation, then, we take the fraction of successes in the series of past trials, that is, the

relative frequency of successes, as the desired objective correlate of our sense of expectancy. This definition of probability makes the sum of the probabilities of a complete set of mutually exclusive events equal to unity, thus providing our metric with a convenient normalization.

The classic rule of Laplace is applicable to another standard class of situations. This second rule presupposes that the information available and relevant to the occurrence of an event of interest K implies that one or another of a set of N mutually exclusive elementary events E_1, E_2, \dots, E_N will take place and that the relation of the evidence to the events $E_1, E_2 \dots, E_N$ is symmetric, every indication of an event E_k being matched by a corresponding indication of every other member of the set. It is further assumed that ν of the events $E_1, E_2 \dots, E_N$ imply that K will occur, while $N - \nu$ imply that it will not occur. Each of the elementary events is then assigned the probability $1/N$ while the probability of the occurrence of K is reckoned to be ν/N , that is, the ratio of the number of favorable possibilities to the total number of possibilities. Here again the probability is a proper fraction and the sum of the probabilities of an exhaustive set of mutually exclusive events is automatically normalized to unity.

The evidence admitted as a basis for the application of the rule of Laplace may be directly statistical, consisting of the record of a series of trials in which $E_1, E_2 \dots, E_N$ have occurred with equal frequency. Other, and less compelling, types of evidence are also admissible, however, such as knowledge of the external symmetry of the six faces of a die, knowledge of the principles of physics, or familiarity with the outcome of a series of trials of a different experiment bearing some similarity to the one in prospect. If the evidence is directly statistical, our second rule for calculating probabilities is justified by its evident equivalence to the first. If the evidence is not directly statistical, it is still possible to give the rule of Laplace a statistical interpretation by saying that the computed probability is the best available estimate of an assumed limiting frequency of successes in a long series of similar future trials, provided that the experiment in question is susceptible of a long series of repetitions. If the experiment is essentially not

repeatable we can only say that the probability is a number calculated from the evidence in a symmetrical situation in just the way we would estimate the limiting frequency of a series of trials if such trials were possible. If the evidence used as a basis for the application of the rule of Laplace does not derive from direct trials we refer to the computed probabilities as *a priori*.

The phrase "best available estimate of an assumed limiting frequency" is suggestive rather than definitive and needs to be interpreted with care. If we make such a "best available estimate," are we then to expect that an experimental series of trials will give approximate confirmation to our estimate? In other words, "Is any capacity to predict involved in the computation of such *a priori* probabilities?" It seems to me that the answer is "No" if the series of trials is made in the usual way, but can be made "Yes" by appropriate modification of the procedure.

Consider the time-honored problem of tossing a cubical die. Let the problem be to evaluate the probability of throwing a six at the first trial, using as evidence the external symmetry of the cube and some familiarity with the principles of mechanics. No evidence for or against a possible loading of the die is given. The symmetry of the cubical form is marred by the spots which mark the faces, but we assume that our knowledge of physics leads us—whether correctly or not—to regard this type of asymmetry as irrelevant to the outcome of the throw. If the die is loaded, we have no information helping us to guess which faces will be favored by the loading. Hence the evidence is symmetric with respect to the six faces, the rule of Laplace is applicable and the desired single-event probability is $1/6$. Can we pass on from the single-event probability to a confident statement about relative frequencies in a series of trials? Can we predict, for example, that if this die is thrown a thousand times a four will turn up with a relative frequency of approximately $1/6$? Not at all. It is quite possible that the die is loaded and that, in consequence, the relative frequencies of the different faces in a long series will be unequal. Hence we have a definite reason to doubt the suggested prediction.

On the other hand there is a similar prediction that we *can* make. Let us consider a long series of throws to be made with a succession of different

dice loaded in any way, but so prepared that the relationship between the loading and the numbering of the faces is accidental. Care is to be taken to insure that the number thrown is not partially controlled by a "trick" method of tossing. In this case each throw will reproduce the conditions we started with. Furthermore, there is good reason to anticipate that the relative frequency of the four will actually be close to $1/6$; for in this case the successive throws are truly independent and any sequence of numbers in the long series must be reckoned as of equal *a priori* probability with any other. A sequence in which one of the face numbers predominates is neither more nor less likely than one in which all faces are equally represented. Hence the rule of Laplace is applicable if we choose the various possible sequences as elementary events of equal likelihood. But the number of sequences that will give approximately equal relative frequencies to the six face numbers is enormous in comparison with the number that distinctly favor one or another of the six. Consequently the *a priori* probability of the event, "sensibly equal frequencies in the long series for each of the six face numbers," is very nearly equal to unity.⁷ As a result of this high *a priori* probability we confidently expect approximate equality of the relative frequencies. The same conclusion can be drawn in the same way if we have only one die, but have made suitable physical tests of the center of gravity and the moment of inertia to establish the fact that the cube is not loaded.

This practical example shows the extent to which *a priori* probability can be used for prediction. At the same time it brings out the great force which inheres in calculations of *a priori* probability when they yield a result very close to unity or zero. *The calculation of an a priori probability is merely an analysis of possibilities. Nevertheless, when the analysis shows that there are a million equally plausible possibilities in favor of an event K to one against it, we do feel compelled to accept the occurrence of K as a practical certainty.* In fact, the psychological force of a single-event probability calculated from any amount of statistics is not a whit different from the psychological force of an equal *a priori* probability. If we

are confronted with a single event and not a series of similar trials, the adjustment to the evidence is the same in the two cases and the procedure by which we have drawn the conclusion is irrelevant. The reader will be convinced of this, I believe, if he asks himself the question, "Would I have any preference between placing a bet on an event with a probability of $1/6$ determined by the statistics of repeated trials and placing the same bet on throwing a three at the first trial with a cubical die whose loading, if any, is unknown?"

A priori probability is frequently criticized as ambiguous, since it may happen that there is more than one way in which a set of mutually exclusive events can be constructed such that one member of the set or another must be an outcome of the experiment we are about to make and such that there is no external reason for preferring one member of the set to another. Using different coordinate systems or different points of view we may arrive at a variety of schemes each of which can make a plausible claim of symmetry. A brief discussion of one or two cases of this sort is given in my previous paper.⁸ Suffice it to note here that in some cases of this type it will be found on close examination that one of the possible schemes has superior claims to reality and validity over any of the others. In other cases a certain amount of arbitrariness will inevitably enter into the choice of the scheme to be used.

In my opinion the value of a computation of *a priori* probabilities is by no means completely lost by the appearance of an arbitrary element in the way we treat the evidence. In fact, it may be worth while in cases where the symmetry required for the Laplace rule is definitely absent to assign unequal, but "reasonable," probability estimates whose sum is unity to the elements of a set of mutually exclusive events E_1, E_2, \dots, E_N and to use these as the basis for the computation of the probability of some secondary event K . Such an assignment cannot be reduced to a standardized mental operation, but is arbitrary. Nevertheless it can be of considerable value if, for example, one can show that the computed probability of K is sensibly independent of the *a priori* assignment of probabilities to the events E_1, E_2, \dots, E_N , provided that this assignment belongs to the class deemed "reasonable." We shall meet an example

⁷ This conclusion is really a special case of the Bernoulli theorem.

of this sort of thing in the last section of this paper.

Closely related to the first type of case in which single-event probabilities can be computed is that in which we have available as evidence for an event of interest the results of a *short* series of previous similar trials. Probabilities in this third type of case are not to be calculated on the basis of the simple rule used for the long series, but we may apply the common term *inductive probability* to the number obtained in each case.

The need for a modification of the relative frequency formula used for a long statistical series when we are confronted with a short series is evident if we consider the case where a single trial has been made. Here the relative frequency is unity if a success has been scored, but we cannot conclude in that case that a second success on the next trial is assured.

The formula used to compute a short series inductive probability must bridge the gap between whatever *a priori* probability exists before any trials are made and the authoritative empirical probability which comes into existence when the series of trials has become so long that *a priori* considerations no longer carry any weight. Thus the formula must vary with the information available at the beginning of the series of trials. In a few special cases (sampling problems) the *a priori* information is of such a character that, with the aid of Bayes' theorem, it is possible to handle the entire problem as an application of the rule of Laplace. No general solution of the short inductive series type of problem is possible, however, and in most cases some arbitrary assignment of *a priori* probabilities must be made in order to arrive at any sort of approximate solution.³ The exact evaluation of probabilities of this class is happily of little moment since the *a priori* element in the situation is rapidly swamped by the empirical element as the series of available trials is prolonged. In practice we can be satisfied to identify the probability with the relative frequency in a fairly long series of trials, neglecting entirely all *a priori* evidence.

OBJECTIVE PROBABILITY

Primary, or single-event, probability is based in general on knowledge that may be incomplete. It

is applicable even in cases where repeated direct trial is impossible. On the other hand, *objective probability* is a construct that results from the projection of primary probability into the external world in cases where our information carries with it at least the illusion of completeness and where we have to do with an experiment or process which is susceptible of practically infinite repetition.

In general, when we make a series of direct trials of an experiment of uncertain outcome we allow the outcome of each trial to correct our expectation of success for the next time. Thus the subjective probability is progressively altered as the statistics of success and failure flow in. There are cases, however, where the subjective probability is not appreciably altered by the results of direct experiments. In such cases the probability is said to reduce to a *chance*. This reduction may result from the completeness of our *a priori* information, as in the case of a cubical die, or coin, that has been subjected to complete and careful physical tests which prove that it is not loaded. In such a case we tend to disregard the evidence of a series of trial throws, and the subjective probability remains constant during the progress of such a series. A similar situation may result from an exceptionally prolonged series of direct experiments. Such trials may add to our information by indicating factors affecting the outcome which can be brought under control in such a way as to reduce the scattering in the results. They also build up in the long run a stable primary probability. Thus there comes a time when further refinement of experimental technique seems impractical, when the scattering of the results of a succession of trials shows internal evidence of being truly haphazard, and when the relative frequency of successes is established within such narrow limits that further experimentation adds nothing of significance to our information. Again the subjective probability is reduced to a chance.

Under such circumstances the arbitrary element in single-event probability is reduced to a minimum and we tend to interpret the remaining uncertainty in the outcome of the individual experiment as an objective property of the external world. Our probability judgment is thought of as a reflection of external natural law

rather than as a result of a rational evaluation of imperfect evidence. The partial convergence of the sequence of observed relative frequencies is extrapolated into perfect convergence in an imaginary infinite extension of that sequence. The imaginary limit of the relative frequency is interpreted as the "true chance" which our finite series of experiments enables us to approximate. This is *objective probability*.

Or, if our probability judgment is based on the application of the Laplace rule to a gambling device of whose honesty we are convinced, we can justify our confidence in a limiting relative frequency by means of the Bernoulli theorem.⁸ This theorem is based on the application of single-event probability to a range of relative frequencies in a long series of trials where the success of the individual trials has a constant probability p . The theorem shows that the expectation of a discrepancy greater than an arbitrarily small positive quantity ϵ between the *a priori* chance p and the relative frequency of successes in a series of N trials can be made as small as we please by choosing N sufficiently large. Because we regard events of zero *a priori* probability as practically certain not to occur, we conclude that in a sufficiently long series of trials the deviation of the relative frequency of successes from the constant probability p will be less than any preassigned value of ϵ . In other words, we are convinced that the sequence of relative frequencies *would* converge if an infinite series of trials *could* be made! To be sure, no experimentation will ever quite convince us that the symmetry of a gambling device is absolutely perfect. Hence we cannot be sure of the exact value of the chance p . Nevertheless, we find it plausible and convenient to believe in the ultimate convergence of the hypothetical infinite sequence of relative frequencies on a value which does not differ appreciably from our estimate. A finite but extended series of trials will ordinarily give qualitative support to this belief.

At this point some captious reader may wish to inquire about the operational meaning of the claim that a limit exists to a hypothetical infinite sequence of numbers which have not been actually evaluated and are not defined by any

mathematical rule. I must either refer such an inquirer to the supporters of the view that the only *true* definition of probability is the frequency-in-a-collective definition, or else confess that for my own part I regard the statement as useful verbalism, that is, a sentence which does not carry with it any obvious unique operational significance. Nevertheless, the circumstances under which the statement is acceptable have been sufficiently explained in the foregoing pages. A description of these circumstances together with an account of the practical consequences which flow from the acceptance of the verbalism define its operational content sufficiently. The practical consequences are, so far as I can see, fundamentally verbal and psychological. Having accepted the existence of the limit we are entitled to talk about it and to introduce it into physical theory as a symbolic construct.⁹ We can also work out an appropriate calculus for the derivation of fresh probabilities from given probabilities on the basis of the limiting frequency construct. Methods so devised are similar to those appropriate to the treatment of primary probabilities based on finite collectives, but embody helpful simplifying approximations. In applications we always return to the finite collective which we think of as an approximation of the ideal infinite one, just as the small spot made on paper by a sharp pencil approximates the ideal point of the mathematicians with its sharply defined coordinates. Thus the concept of objective probability, verbally defined as the limit of the relative frequency in an infinite collective, is an extremely useful mental aid in the treatment of statistical problems despite the fact that it seems not to be absolutely indispensable.

THE MEANING OF PROBABILITY IN STATISTICAL MECHANICS

Having built up in outline a general positive theory of primary and objective probabilities, it remains for the writer to justify the claim made in the introduction of this paper that single-event probability has an important part to play in the development of the fundamental principles of statistical mechanics.

The basic problem of this theory is to prove that every large-scale isolated mechanical system

⁸ See, for example, J. V. Uspensky, *Introduction to mathematical probability* (New York, 1937), p. 96.

⁹ See H. Margenau, Phil. Sci. 2, 1 (1935).

—sample of matter—tends to approach an ultimate steady state of thermodynamic equilibrium in which its macroscopic properties are constant.¹⁰ Inseparable from this problem is the business of developing a statistical model of the state of thermodynamic equilibrium.

The model at which one arrives is a micro-canonical Gibbsian ensemble of independent systems defined in the classical case by a distribution of representative points in the Gibbs phase-space. In the quantum-mechanical form of the theory the ensemble is described by a statistical matrix. In either case we could equally well use single-event probability or objective probability to relate the model to the individual system under consideration. Thus we can interpret the density of the representative points of the model as a distribution of single-event probability (chance) for the phase of a system prepared so as to be in thermodynamic equilibrium, or we can regard this distribution as the description of the distribution of phases in an imaginary series of experiments in which we repeatedly prepare a system in thermodynamic equilibrium and then measure its phase. In the latter case the objective probability of a given element of phase volume is equal to the product of the volume and the Gibbs coefficient of probability.

No one seriously doubts the validity of the model, but it has never been rigorously established in either classical theory or quantum theory. Proof must depend upon one or another of the various forms of ergodic theorem, and it is quite clear that no form of this theorem holds for all varieties of Hamiltonian function. There is no way to test the Hamiltonian of any given sample of matter. Hence we are thrown back on the presumption that Hamiltonian functions which do not meet our requirements are very *improbable*. In the quantum-mechanical form of the theory¹¹ one circumvents the difficulty by group integration over all allowed forms of a transformation matrix dependent on the Hamiltonian

function. As I have previously noted,¹¹ this amounts to setting up a continuous distribution of *a priori* probability for the different forms of transformation matrix. There can be no question of setting up an objective probability distribution for these matrices, and hence there is apparently no way of eliminating this resort to the use of *a priori* probability. Fortunately the argument seems convincing as it stands.

The difficulties encountered in attempts to prove the ergodic theorem for classical mechanics indicate the need for a similar use of *a priori* probability in this connection also.¹²

Aside from the ergodic theorem the proof of the validity of the microcanonical ensemble as a model for the state of thermodynamic equilibrium is facilitated by the use of the concept of *a priori* probability.

In his famous book on statistical mechanics Gibbs¹³ based the discussion on the proposition that, if an ensemble of systems be started off in such a manner that the density of their representative points is initially distributed in any quasi-continuous manner over the infinitely thin shell between two ergodic surfaces—that is, surfaces of constant energy—the motion in time will ultimately distribute these points uniformly over the entire shell and thus convert the ensemble to the form we call microcanonical. This proposition is closely related to the ergodic theorem and, despite lack of rigorous proof, has been generally regarded as affording the only acceptable explanation of the facts of thermodynamics from the classical point of view.

Gibbs merely traced analogies between the properties of these ensembles of hypothetical systems and the properties ascribed to individual systems in thermodynamics. He cautiously avoided any attempt to use the theory of ensembles to *prove* the laws of thermodynamics; yet such proof is clearly needed to round out the relation between statistical mechanics and thermodynamics. The Boltzmann method of bridging the hiatus is to use the properties of the ensembles

¹⁰ In this statement I adopt the usual nonstatistical interpretation of thermodynamics and set aside for the purpose of the present argument the statistical reformulation of thermodynamics which I have recently advocated. See Phys. Rev. **56**, 1013–1023, 1146–1164 (1939).

¹¹ See J. von Neumann, Zeits. f. physik **57**, 30 (1929); E. C. Kemble, reference 10.

¹² I have to confess that I have never mastered the papers of Birkhoff and von Neumann on this subject but can report that, in the opinion of Professor Birkhoff, the theorem is by no means established in a form adequate to the needs of statistical mechanics.

¹³ *Elementary principles in statistical mechanics* (Scribner, 1902; Yale Univ. Press, 1902); reprinted in the *Collected works of J. Willard Gibbs* (Longmans Green, 1928), vol. II.

to show that the time average of any function of the coordinates of a single system moving in accordance with the laws of motion asymptotically approaches the ensemble average of the same function for a microcanonical ensemble of the same energy. The totality of states through which the isolated system runs in infinite time can then be identified with the collective required for the creation of an objective probability. The probability of any range of phases calculated from the density function for the microcanonical ensemble becomes identical with the probability calculated from the aforementioned collective by the relative frequency method.

An alternative procedure for bridging the hiatus that seems to me even more instructive is to consider the single-event probability of different portions of phase-space for a sample of matter on which we have just made measurements to fix its state as exactly as possible. Because of the crudity of macroscopic measurements, the phase can never be precisely fixed; but these measurements do drastically limit the portion of phase-space in which the representative point of the system can be located. There is no basis for determining a unique initial distribution of probability, but our observations nevertheless provide us with tests by which in principle we can distinguish between probability distributions compatible with our data and others that are not compatible. Let us now suppose that, for the purpose of the argument, we *arbitrarily* assume the validity of an allowed distribution of *a priori* probability. This can be matched by the initial density function of a hypothetical ensemble. As time goes on the density function is transformed by the natural motions of the representative points. Corresponding to each initial element of volume there is a transformed element which contains the representative points of the same systems and must have the same probability. Hence the transformed density function after any given interval of time determines the probability distribution appropriate to the later time. But, by hypothesis, *every* continuously distributed ensemble restricted to an ergodic shell ultimately approaches microcanonical form. Consequently *every* initially continuous distribution of single-event probability in such a shell tends asymptotically to the uniformity characteristic

of the density of a microcanonical ensemble. It follows that any two equal volumes of the shell become equally probable in the long run, independent of the macroscopically determined initial state and of the particular choice of *a priori* probability distribution with which we start. We conclude that after sufficient time a division of the shell into equal volume elements can be made the basis for the selection of a set of mutually exclusive events symmetrically related to our information and suitable to the application for the rule of Laplace.

The macroscopic state of the system is fixed by the so-called "normal properties" of the system, that is, by functions of the coordinates which have values very close to their averages over all but an infinitesimal portion of the shell volume. The probability that any one of these will have a value appreciably different from its average is then sensibly equal to zero, and we can predict with confidence that such anomalous values will not occur. Thus we infer the existence of an asymptotic steady macroscopic state.

Let us now ask ourselves whether this system of single-event probabilities is a system of chances from which we can predict the distribution in phase-space at the end of a specified time for the representative points of a collective ensemble of identical independent systems started off in the same way with macroscopic tools and then isolated for a very long period. I think we can say that it is. We have already seen, in the case of a cubical die, that probabilities become chances when we are convinced that there can be no factor in the situation affecting the result of a series of trials which is unknown to us but which would alter our estimate of likelihood if we knew of it. The only conceivable factor of this kind in the present case would be a cousin of the Maxwell demon who could control the initial phases of the successive systems in our ensemble in such a manner as to give them a discontinuous distribution in phase-space. Thus, if all members of the collective were started off in exactly the same phase, the representative points would adhere permanently and, after any length of time, the ensemble would be as far from microcanonical as at the beginning. This possibility is one which I believe we can properly reject. If so, we can say that the single-event

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probabilities associated with the microcanonical ensemble are also chances which can be interpreted as objective probabilities for the type of collective just discussed.

The foregoing argument leads us directly to the first of the two schemes for relating the model ensemble to the individual thermodynamic system in hand. In this case the primary function of the Gibbsian ensemble is to give intuitive assistance to the study of the transformation of single-event probabilities in time. The ensembles are in any case hypothetical and so afford no empirical basis for the evaluation of relative frequencies.

Superficially, the use of *a priori* probability here made in establishing the validity of the microcanonical ensemble as a model bears a resemblance to the procedure adopted by Professor Tolman¹⁴ and previously criticized by me.¹⁵ Hence I should, perhaps, make clear the distinction between the two points of view. Tolman introduces probability as a *physical hypothesis* into the formulation of the fundamental principles of statistical mechanics in such a manner as to eliminate completely the need for the ergodic hypothesis or any equivalent for it. His broad assumption is that any two equal volumes in phase-space are equally probable, provided our actual knowledge of the system is equally well represented by phases belonging to the different volumes. He then proceeds directly to the conclusion that the microcanonical ensemble must be the "representation" appropriate to a system whose energy is known but concerning which we have no other information. Finally, he infers that a system of known energy isolated for a long time must be reckoned as one concerning whose phase we have no knowledge beyond the specification of the ergodic surface on which it moves. Hence the state of such a system is to be "represented" by a microcanonical ensemble.

If Tolman's hypothesis referred to the setting up of a scheme of *a priori* probabilities, I should have no criticism of it except to note that the uniqueness of the scheme may require discussion. The use of the term "hypothesis" and the fact

that his discussion of the validity of the microcanonical ensemble for the state of thermodynamic equilibrium is terminated by the foregoing argument both suggest, however, that Tolman is using the term probability in the objective as well as in the *a priori* sense. A physical hypothesis is ordinarily understood to be an assumption that is in principle susceptible of experimental verification. A hypothesis regarding probability inserted into the development of a physical theory is then presumably one referring to the only kind of probability susceptible of experimental verification, namely, objective probability.

If my interpretation of the Tolman argument is correct, it is subject to criticism on two counts. In the first place, it substitutes for the ergodic postulate, which we may still hope to convert into a theorem, a far more sweeping assumption that the author has no thought of proving. This in effect amounts to giving up all hope of proving that the laws of thermodynamics are a logical consequence of the laws of mechanics and the complexity of the structure of matter. In the second place, the Tolman argument overlooks the conditions under which an *a priori* probability can be converted into an objective probability.

An objective probability exists only when the statistical distribution of results from a very long series of identical experiments is unique. It cannot exist unless the experimental procedure is sufficiently well defined so that any two observers following the instructions must necessarily obtain the same statistical result. It makes sense to formulate a single-event probability for a concrete experimental trial when one lacks information which may have an important bearing on the result, but to speak of the indefinite repetition of such a trial is to use an ambiguous phrase. Hence we can attach no unique objective probability to it. Thus, whatever our information about its origin, it is reasonable to speak of the single-event probability that a particular pie on Mrs. Smith's pantry shelf is cherry. It is not reasonable, however, to make a hypothesis regarding the relative frequency of cherry fillings in an indefinitely long series of pies without specifying anything regarding the way in which the pies are to be gathered—whether from New England or California, and so forth.

¹⁴ R. C. Tolman, *The principles of statistical mechanics* (Oxford, 1938), secs. 23 and 84. A similar procedure has been proposed by Elsasser, Phys. Rev. **52**, 987–999 (1937), sec. 3.

¹⁵ See the reference in footnote 10.

The required definition of experimental procedure exists for systems which are to be isolated for a very long time, and in that case, as I have already indicated, we may infer the existence of an objective probability. On the other hand, it is illogical to suppose that a thermodynamic system concerning whose state one has slight information is correlated with an objective distribution of probability in phase-space, for the experiment of choosing another system concerning which one has the same information is too vague to define unique repetition.

SUMMARY

This paper is an attempt to present a view of the probability concept radically different from that supported by Professor Bergmann's article in this journal.¹ The crucial point of difference between Professor Bergmann and myself lies in my rejection of an axiom basic to his point of view, namely, the axiom that there is no scientific

use for a form of the probability concept which cannot be subjected to experimental verification. Because he does not consider this axiom open to question there is no occasion for a point-by-point reference to his argument. In effect, the whole of the present paper is an attempt to prove the unwisdom of this superficially very plausible postulate.

On the basis of the aforementioned rejection I propose that single-event probability—in particular, that type of single-event probability commonly designated as *a priori*—should be included despite its nontestability in the list of essential scientific concepts. This view is supported (a) by an analysis of the psychological background and significance of the probability of single events, (b) by a consideration of the relation between objective probability and the probability of single events, and (c) by a discussion of the essential role of single-event probability in statistical mechanics.

An Experiment with the Current Pendulum

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THE current pendulum has proved to be an excellent experiment for the advanced undergraduate dynamics laboratory. It correlates the work of the course in dynamics with that in electrical measurements, and gives useful practice in making approximations, in empirical curve fitting and in application of the theory of small vibrations. Furthermore Lagrange's equations of motion may be effectively taught to undergraduates by means of this experiment.

For undergraduates it is perhaps confusing to derive the Lagrange equations from a variation principle. It is better to transform Newton's second law to generalized coordinates. The result for the k th coordinate q_k is

$$\frac{d}{dt} \left(\frac{\partial T_1}{\partial q_k} \right) = \frac{\partial T_1}{\partial q_k} - \frac{\partial V}{\partial q_k} + Q_k, \quad (1)$$

where T_1 is the kinetic energy, V is the potential energy and Q_k is the dissipative or frictional force. The left-hand member is the time-rate of change of the so-called generalized momentum. The first term on the right is sometimes called the "fictitious force." The second term on the right represents the part of the force which may be derived from a potential energy function, that is, the conservative force.

The last term on the right is the nonconservative generalized force. By introducing the Lagrange function L and putting Q_k equal to zero one obtains the form

$$\frac{d}{dt} \left(\frac{\partial L}{\partial q_k} \right) - \frac{\partial L}{\partial q_k} = 0, \quad (2)$$

which holds for a conservative system.

From an analytic standpoint the current pendulum provides an excellent means for teaching the Lagrange equations because the potential energy expression contains not only a term due to the position of a movable coil in the gravitational field of the earth but also terms due to its position in the magnetic field of two stationary coils. The student thus has an opportunity to combine his knowledge of dynamics and electricity.¹

DESCRIPTION OF THE APPARATUS

Figure 1 is a schematic diagram of the apparatus. Each of the fixed coils consists of 111 turns of No. 16 double cotton covered magnet

¹ A simpler version of the present experiment is described by Calthrop, Am. J. Phys. (Am. Phys. T.) 3, 32 (1935); it has been performed successfully by students in our advanced dynamics laboratory during the past two years.

wire wound on a circular wooden disk approximately 28 cm in diameter. Larger diameter circular pressboard sheets are cut and fastened to the wooden form concentrically to provide a channel of rectangular cross section for the winding. The winding is put on in smooth even layers, the jump from each layer to the next being made always at the same point. Considerable care should be exercised in the con-

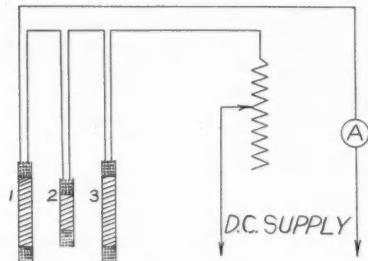


FIG. 1. Diagram of the apparatus.

struction of the coils since one needs to measure accurately the mean radius of each coil and the mean distance between them. The movable coil 2 is similar in construction to the stationary coils but is wound on a smaller wooden annular form of approximately 20 cm diameter with annular pressboard ends in order to make its mass as small as possible. It is hung symmetrically between the two fixed coils by means of two fish lines, the upper ends of which are fastened to screw eyes located about 1 m above the coil in a rigid wooden frame. A certain amount of freedom of adjustment for height is necessary. The conducting wires—No. 22 copper—hang loosely around the bifilar supporting lines and pass loosely through the screw eyes which provide supports for them. In addition, a fairly long, coiled pigtail is made in each conducting wire before fastening it to the rigid frame in order to minimize the force it exerts on the pendulum. There must be no iron in the immediate vicinity of the coil system. The coils are connected in series with the proper polarity such that repulsion exists between the movable coil and each of its fixed neighbors. The position of equilibrium is thus midway between the fixed coils for every value of the current. If the movable coil is properly centered it will not move when the current is turned on suddenly.

THEORY

Let c represent the absolute value of the distance between the middle of the movable coil and the middle of either of the fixed coils when the movable coil is in its equilibrium position; this is half the distance from the middle of one fixed coil to the middle of the other fixed coil. Let x represent the displacement of the center of mass of the movable coil measured horizontally from its equilibrium position at any instant of time. Furthermore, let m be the mass of this coil; h , the distance from its center of mass to the supporting screw eyes; M_{12} , the coefficient of mutual inductance between the fixed coil 1 and the movable coil 2; M_{23} the similar coefficient between the movable coil 2 and the fixed coil 3; I , the current; a , the mean radius of the smaller (movable) coil; and b , the mean radius of either of the identical fixed coils. The so-called permeability of empty space, μ_0 , has the value $4\pi \times 10^{-7}$ in the rationalized mks units used throughout this article.

The potential energy of the system is²

$$V = mgh \left(1 - \cos \frac{x}{h} \right) + M_{12}I^2 + M_{23}I^2 + \text{const},$$

where the $\frac{1}{2}LI^2$ terms which represent the magnetic self-energy of the currents have been incorporated in the constant term and where x/h is the very small angle which the bifilar suspensions make with the vertical.

The expression for the coefficient of mutual inductance between two coaxial circular loops of radii a and b with their centers a distance c_1 apart is given by Neumann's formula,³

$$M = \mu_0(ab)^{\frac{1}{2}}[(2/k - k)K - 2E/k],$$

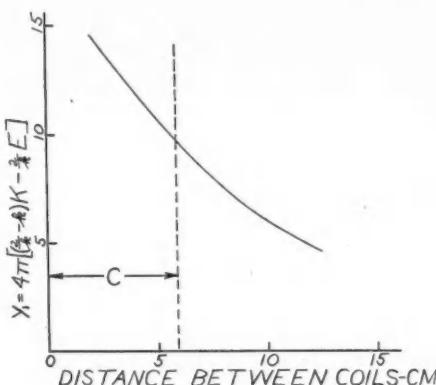
where k^2 is $4ab[(a+b)^2 + c_1^2]^{-1}$ and K and E are complete elliptic integrals of the first and second kinds,

$$K = \int_0^{\pi/2} (1 - k^2 \sin^2 \varphi)^{-\frac{1}{2}} d\varphi,$$

$$E = \int_0^{\pi/2} (1 - k^2 \sin^2 \varphi)^{\frac{1}{2}} d\varphi.$$

² Harnwell, *Principles of electricity and electromagnetism* (McGraw-Hill), ed. 1, pp. 298-299.

³ Reference 2, p. 304.

FIG. 2. Graph of y_1 versus $c+x$.

These can be found from tables of functions. The foregoing expression for M will give approximately the coefficient of mutual inductance between two coils if multiplied by the product $N_1 N_2$ of the numbers of turns in the coils. The approximation consists in assuming that all of the turns are concentrated at the center of the winding channel in each coil.

The graph of

$$4\pi \left[\left(\frac{2}{k} - k \right) K - \frac{2}{k} E \right],$$

referred to hereafter as y_1 , as a function of c_1 , is shown in Fig. 2. The function y_1 is dimensionless and is proportional to the potential energy associated with the repulsive force between coils 1 and 2. We need to express k as a function of the variable x . This is done by writing

$$k^2 = 4ab[(a+b)^2 + (c+x)^2]^{-1}.$$

A similar expression can be obtained for y_2 , the function associated with the repulsive force between coils 2 and 3, merely by changing x to $-x$ in y_1 , as can be seen from the symmetry of the coil system. The sum of the two functions, which is proportional to the total potential energy associated with electromagnetic forces, is a minimum at x equal to zero, as is to be expected since this is the equilibrium position.

The function y_1 is too cumbersome to handle as it stands. Since one is interested in small vibrations about the equilibrium position it is

easiest to fit an empirical formula to the part of the graph that is in the immediate vicinity of this position. In Fig. 3, $\ln y_1$ is plotted as a function of $c+x$. Since the graph is a straight line the proper empirical formula to fit to the curve of Fig. 2 is of the form $y_1 = k_1 a_1^{-x}$, where k_1 and a_1 are constants. If y_1 is expanded into a Taylor's series the result is, using terms as far as x^2 ,

$$y_1 = k_1 \left[1 - x \ln a_1 + \frac{1}{2!} (-x \ln a_1)^2 \right].$$

A similar result is found for y_2 , namely,

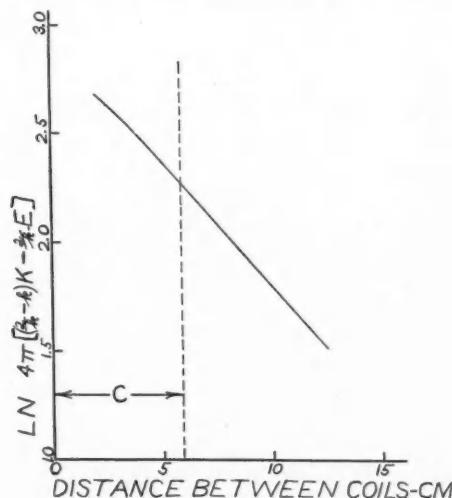
$$y_2 = k_1 \left[1 + x \ln a_1 + \frac{1}{2!} (x \ln a_1)^2 \right].$$

The potential energy (except for a constant of proportionality) associated with the electromagnetic field is thus

$$y = y_1 + y_2 = k_1 [2 + (x \ln a_1)^2].$$

The expression for the gravitational potential energy when expanded into a Taylor's series, again using terms as far as x^2 , is $(mg/2h)x^2$. The complete potential energy is therefore represented by

$$V = (ab)^{\frac{1}{2}} N_1 N_2 I^2 \cdot 10^{-7} k_1 (x^2 \ln^2 a_1 + 2) + (mg/2h)x^2.$$

FIG. 3. Graph of $\ln y_1$ versus $c+x$.

The kinetic energy is given by the expression

$$T_1 = \frac{1}{2}(J/h^2)\dot{x}^2,$$

where J , the moment of inertia of the movable coil about an axis through the supporting screw eyes, is very nearly equal to mh^2 . Thus the expression for the kinetic energy takes the simpler form,

$$T_1 = \frac{1}{2}m\dot{x}^2.$$

Substitution of the expressions for V and T_1 in Eq. (2) yields the equation of motion,

$$m\ddot{x} + [2(ab)^{\frac{1}{2}}N_1N_2 \cdot 10^{-7}I^2k_1 \ln^2 a_1 + mg/h]x = 0,$$

which represents simple harmonic vibrations of period

$$T = 2\pi \left[\frac{m}{2(ab)^{\frac{1}{2}}N_1N_2 \cdot 10^{-7}I^2k_1 \ln^2 a_1 + mg/h} \right]^{\frac{1}{2}}.$$

For zero current this expression for the period reduces to that for an ideal gravity pendulum,

$$T_0 = 2\pi(h/g)^{\frac{1}{2}}.$$

By introducing T_0 into the expression for T and solving for I^2 one obtains the working equation,

$$I^2 = \frac{2\pi^2 \cdot 10^7 m}{(ab)^{\frac{1}{2}}N_1N_2k_1 \ln^2 a_1} \left(\frac{1}{T^2} - \frac{1}{T_0^2} \right). \quad (3)$$

TABLE I. Typical data for two values of the equilibrium distance.

$c = 8.10 \text{ cm}$		$c = 7.32 \text{ cm}$	
Pendulum (amp)	Ammeter (amp)	Pendulum (amp)	Ammeter (amp)
1.01	0.99	1.01	1.00
1.86	1.82	1.46	1.45
2.28	2.24	1.88	1.88
2.72	2.65	2.29	2.28
3.45	3.35	2.73	2.70
4.22	4.11	3.14	3.08
4.62	4.48	3.83	3.80
5.00	4.90	4.23	4.17
		4.57	4.50
		4.98	4.92

The value of k_1 is obtained either directly from Fig. 2 or from its Napierian logarithm in Fig. 3. $\ln a_1$ is the negative of the slope of the straight line in Fig. 3. Values of I may then be computed from measured values of T and T_0 . A check on the correctness of these computed currents is obtained by measuring I directly with the ammeter A , Fig. 1.

Table I lists typical results for two values of the equilibrium distance c . The ammeter against which the experimental values were compared was calibrated recently with a Wolf potentiometer and a standard cell. The agreement of the experimental values with the ammeter readings is as good as can be expected in view of the approximations made in the theory.

Available Graduate Appointments and Facilities for Advanced Study

STUDENTS who are planning to engage in graduate work next year should consult the bi-yearly survey of opportunities for advanced study in 111 American and Canadian institutions that appeared in the December 1940 and February 1941 issues of this journal. The following additional information concerning three institutions is available:

Johns Hopkins University. 4(6) junior instructors, 2 hr/wk recitation, 2 afternoon/wk lab. asst., \$1000 less \$336 t. and f.; 3 night school instructors, 2 evenings/wk, \$700; 1 night school instructor, astronomy, 2 hr/wk, \$380; 6 assistants, 2 afternoons/wk lab. asst., remission of t. and f.; 3 President's fund scholars, \$1000 less \$336 t. and f. (If

holder shows promise, this scholarship may be renewed twice and followed by a 1-yr appointment as *instructor*); several university scholars, remission of \$300 t.; 4 Quincy scholars, \$160 (May be held by assistants and University scholars).

Michigan State College, Prof. T. H. Osgood, East Lansing, Mich. No form. 4(6) graduate assistants, 5-6 periods/wk lab. or equiv., \$600 for 10 mo., no f. R: x-rays, spectroscopy, geophysics, supersonics.

University of Rochester. R: electron emission; nuclear physics; optics; biophysics; theoretical physics (nuclear, radiation, solids).

A Demonstration in Kinetic Theory

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In elementary kinetic theory it is shown that $v_{Av}^2 = 3kT/m$, where v_{Av} is the root-mean-square velocity, k is the Boltzmann constant, m is the mass of the molecule and T is the absolute temperature. Suppose a small plane is moving with a velocity u relative to a container maintained at a temperature T whose surface area is very large compared with that of the moving plane. For simplicity let the molecules all have the same speed v , the direction of motion being perfectly random with respect to the walls of the container. If the collisions with the surface of the plane are elastic, then the average of the velocity squared for the molecules moving toward and rebounding from the plane will be

$$[(-(\mathbf{v} - \mathbf{u}) \cdot (-\mathbf{v} - \mathbf{u}))]_{Av} = (\mathbf{v}^2 + 2\mathbf{u} \cdot \mathbf{v} + \mathbf{u}^2)_{Av} \\ = v_{Av}^2 + 2u \cdot v_{Av}$$

Obviously only those molecules whose component of \mathbf{v} in the direction of \mathbf{u} is less than u will strike the advancing face. These molecules are included in the dotted surface $CDEFG$ of Fig. 1. If $u \ll v$ then, for a first approximation, it is sufficient to average the molecules included in the solid surface EFG of Fig. 1. Thus

$$\frac{u \int v u dN}{\int dN} = \frac{u \int_0^{\pi/2} v \cos \theta 2\pi \sin \theta d\theta}{\int_0^{\pi/2} 2\pi \sin \theta d\theta} = \frac{1}{2} uv,$$

where we have assumed that equal numbers of molecules have passed normally through equal areas of the hemisphere. Then the apparent temperature of the advancing face is $T + \Delta T = (mv^2/3k)(1+u/v)$, where we have neglected terms involving $(u/v)^2$; or the temperature is raised by an amount $\Delta T = uT/v$. Similarly, the back side should have its temperature lowered by an equal amount provided there is no heat conduction between the two surfaces. The mean free path has been considered large compared with the dimensions of the advancing plane.

To illustrate this effect, two thermopiles consisting of copper constantan thermocouples of fine wire supported by Lucite spools are rotated by means of a reversible electric motor in a vacuum of 10^{-2} mm-of-mercury at a linear speed of 10^3 to 5×10^3 cm sec $^{-1}$. The mean free path for the air molecules at this pressure is about 1 cm and is of the same order of magnitude as the dimensions of the thermopile, rather than being large compared with the latter. However, the assumptions of the preceding paragraph are sufficiently obeyed to permit a qualitative verification of the effect. It is infeasible to work at lower pressures where the mean free path is longer, for reasons given in the following paragraphs. The speed of rotation of the bar supporting the thermopiles is measured by placing a small permanent magnet on the shaft of the motor and connecting the output of a small coil of 1000 turns mounted nearby to one set of plates of a cathode-ray oscilloscope. The other set of plates is connected to a calibrated

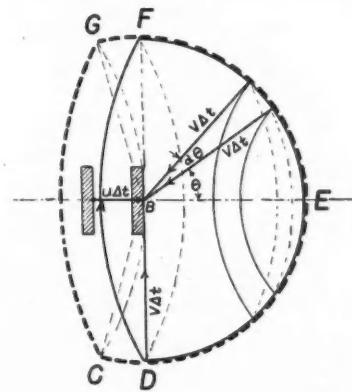


FIG. 1. The average velocity of molecules for an observer on the moving disk. When the disk is at the point A , only the molecules on and moving normal to the dotted section $CDEFG$ of the sphere of radius $v\Delta t$ will hit the advancing face of the small disk when it has moved to the point B in the time interval Δt . The speed of the disk is u and that of the molecule is v . If $u \ll v$, only the solid portion DEF forming a hemisphere need be considered in finding the average velocity of the molecules for an observer moving with the front of the disk.

beat-frequency oscillator, and Lissajous figures are obtained.

Unfortunately, the heat conduction of the Lucite and the wire in the thermocouples is very appreciable; this makes it difficult to obtain quantitative results. However, the magnitude of the temperature difference to be expected can be approximated in the following manner. Let $T + \Delta T'$ be the observed temperature of the advancing face and $T - \Delta T'$ the observed temperature of the back face. The energy in ergs transferred per second from face to face on account of heat conduction is $2B\Delta T'$, where B is the coefficient of conduction ($\text{erg deg}^{-1} \text{ sec}^{-1}$) for the thermopile. About $nva/4$ molecules hit each face per second, where A is the area of the face and n the number of molecules per cubic centimeter. Those at the front face arrive with an energy $(3k/2)(T + \Delta T)$, are absorbed, and then leave with an energy $(3k/2)(T + \Delta T')$, while those at the back face arrive with an energy $(3k/2)(T - \Delta T)$ and leave with an energy $(3k/2)(T - \Delta T')$. The rate of energy transfer from face to face is

$$\frac{3k}{2}[(T + \Delta T) - (T + \Delta T') + (T - \Delta T')] - (T - \Delta T)]nva/4 \text{ erg sec}^{-1}.$$

Equating the two rates of energy change and letting $C = 8B/3knvA$, we find

$$\Delta T' = \frac{T}{C+1} = \frac{uT}{(C+1)v}.$$

In our apparatus (Fig. 2) the dimensionless quantity $C+1$ is about 100, and the value of $\Delta T'$ is observed to be in good agreement with the known values of u , v and T . Since C is inversely proportional to n , a further reduction in pressure would increase C . This increase would make the observed temperature difference $\Delta T'$ so small that it would be difficult to measure. The difference $\Delta T'$ was also found to be roughly proportional to u , as we should expect from the

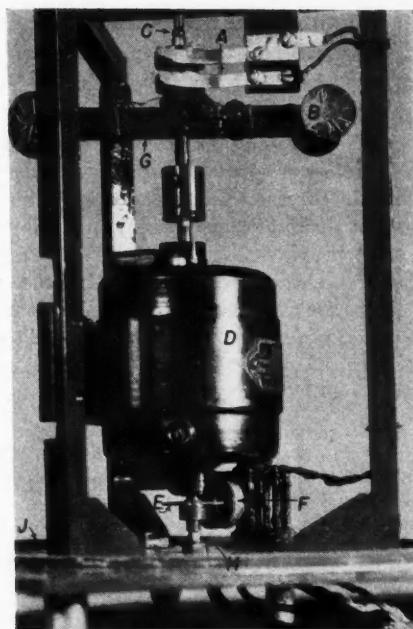


FIG. 2. Velocity of air molecule apparatus: *A*, thermopile slip rings; *B*, thermopile; *C*, ball bearing; *D*, reversible electric motor; *E*, permanent magnet; *F*, 1000-turn coil; *G*, rotating arm; *H*, cone bearing; *J*, rubber gasket. The bell jar which fits over the apparatus is not shown.

equation $\Delta T' = uT/(C+1)v$. To eliminate the thermal emf's caused by the frictional heating of the thermopile commutator brushes, the motor was reversed, thus changing the sign of the emf developed by the thermopile while keeping those of the commutator constant. The effect was found to be of the opposite sign at atmospheric pressure, the advancing face apparently cooling. One explanation which has been suggested to the authors is that, because of the stream motion at these pressures, there is a radial (centrifugal) flow of gas across the faces of the disks. The Bernoulli principle would then account for a local reduction of pressure next to those faces, and the consequent adiabatic expansion of the gas would cool it. This neglects the stream motion over the back face of the disk.

A great nation assailed by war has not only its frontiers to protect, it must also protect its good sense.—ROMAIN ROLLAND.

Should One Stop or Turn in Order to Avoid an Automobile Collision?

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SUPPOSE an automobile collision is impending. If there is not room to stop, can one get out of trouble by turning? Such a question as "Should one stop or turn in order to avoid an automobile collision?" provides excellent material for discussion in physics classes ranging all the way from elementary to advanced ones. The answer to the question depends, of course, on conditions. We can be specific by choosing the following problem:

A man driving an automobile at high speed along a side road suddenly arrives at the intersection of a vacant highway. The side road does not continue beyond the intersection, there being a wall along the far side of the highway. To avoid hitting the wall, the man must either stop in the intersection or turn. With a given coefficient of friction between tires and road surface—that is to say, with a given force available for stopping or turning (or both)—in what path should the man steer so as to miss the wall by the greatest distance—that is, travel the least distance into the highway, the distance being measured parallel to the direction of the original velocity?

We shall make two assumptions: (1) the coefficient of friction sideways is the same as that in the direction of motion;^{1,2} (2) if the man is not

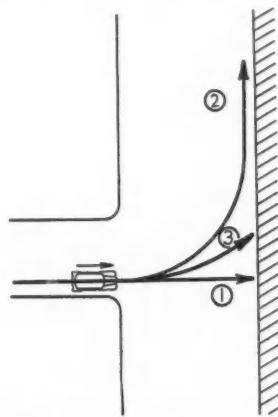


FIG. 1. Diagram showing automobile at an intersection with three possible choices of path.

* Now at Stanford University, Calif.

¹ This assumption is valid for a preliminary consideration of the problem, especially if skidding is ruled out; see, for instance, reference 2, pp. 56-57.

² R. A. Moyer, "Skidding characteristics of tires on roadway surfaces and their relation to highway safety," Bull. 120, Iowa Engineering Experiment Station (Iowa State Coll., 1934).

using the force to provide centripetal acceleration so as to turn, he will use it by putting on the brakes to provide negative acceleration in the direction of motion.

Three choices are open: (1) to steer straight ahead and apply the brakes fully; (2) to turn in a circular arc without braking, using all the available force to produce centripetal acceleration; (3) to choose some combination of (1) and (2), such as turning first and then steering straight, or vice versa, or steering in some spiral path. These three possibilities are indicated in Fig. 1. The second choice will give the smallest possible radius of curvature—the sharpest turn. It is unlikely that even a small group of persons will be unanimous as to the proper choice.

If l is the width of the highway, or the original distance from the car to the wall, and if v_0 is the original speed of the car, we may dispose of choice (2) by writing an expression for the force F_r required to turn a particle of mass m and constant speed v_0 in a circular arc of radius l , namely, $F_r = mv_0^2/l$, and writing an expression for the force F_s required to stop a particle of mass m and speed v_0 in a distance l , namely, $F_s = \frac{1}{2}mv_0^2/l$. It is clear then, that twice as much force will be required for the circular turn as for the straight stop; or that, if the car can be turned in a circular arc without hitting the wall, it can be stopped in only half the distance to the wall.³ Many students who drive cars will be surprised at this simple result.

The choice between the straight stop and the spiral, or combination, path seems more difficult, but the following proof, which takes us away from elementary physics, shows that the proper choice is the straight stop.

To be specific, let us state the problem in mathematical language. A particle of mass m starts from the origin with a velocity \mathbf{v}_0 in the direction of the X -axis. It is acted upon by a force \mathbf{F} of constant magnitude $F = (F_x^2 + F_y^2)^{\frac{1}{2}}$ at a

³ Since these two expressions, or their equivalent, can be found in practically any high school physics textbook, school students should be able to appreciate this part of the discussion.

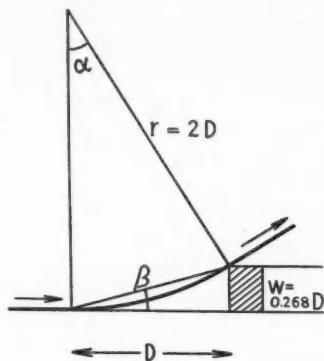


FIG. 2. Path taken by a car dodging an obstacle of width W at a distance D by steering in a circular arc of radius $2D$.

variable angle θ with the X -axis, where θ is a function of x . The problem is to find a function $\theta(x)$ which may be used to calculate the path $y[y=x(x)]$ so that the particle will travel the least distance s in the X -direction while v_x , the x -component of the velocity, is being brought to zero. Note that the distance s will be measured, not along the path, but parallel to the X -axis.

The equation that determines the value of the variable s is

$$\int_0^s F_z dx = \frac{1}{2}mv_s^2 - \frac{1}{2}mv_0^2, \quad (1)$$

or since, at $x=s$, $v_s=0$,

$$\int_0^s F_z dx = -\frac{1}{2}mv_0^2.$$

Now $F_z=F \cos \theta$, and hence our problem is to find the form of the function $\theta(x)$ for which s is a minimum in the equation

$$\int_0^s F \cos \theta dx = -\frac{1}{2}mv_0^2.$$

While in this case the appearance of the variable limit s in the integral will not affect the result, we can circumvent the difficulty of dealing with this limit by making a change in the conditions of the problem. Thus in the equation

$$\int_0^l F \cos \theta dx = \frac{1}{2}m(v_l^2 - v_0^2), \quad (2)$$

we must find $\theta(x)$ such that $v_l^2 - v_0^2$ will be a maximum; or, if v_l is zero, find the path in which to steer so that the car with the largest initial speed can just be stopped.

To solve Eq. (2) we take the variation of both members, realizing that we may determine a maximum, a minimum or a stationary value for the integral. Thus

$$\delta \int_0^l F \cos \theta dx = 0$$

or, since F is of constant magnitude,

$$\delta \int_0^l \cos \theta dx = 0,$$

$$\int_0^l \delta(\cos \theta) dx = 0,$$

$$\int_0^l (-\sin \theta) \delta \theta dx = 0.$$

But $\delta \theta(x)$ is a wholly arbitrary function, and if the integral is to vanish for an arbitrary value of this function, we must have $-\sin \theta(x)=0$, whence $\theta(x)=n\pi$, where n is an integer. Of these solutions only two are distinct: $\theta=0$, and $\theta=180^\circ$. In either case, we see that the path is a straight line in the direction of the initial velocity.

If $\theta=0$, in which case the force is in the same direction as the velocity, we infer that the car will strike the wall with a maximum velocity (not necessarily the largest velocity). If $\theta=180^\circ$, or the force opposes the motion, and v_l is taken as zero, we determine v_0 from the relation $-Fl=\frac{1}{2}mv_0^2$. Hence the solution of the original problem is given by the first of the three choices; namely, to apply the brakes and steer straight ahead.

Now we may modify the problem by supposing the wall to be too close for the car to be stopped in the straight path. Let us seek the path that will give the least component of velocity perpendicular to the wall at the time of impact.

Returning to Eq. (2), fix l and v_0 , where $\frac{1}{2}mv_0^2 > Fl$. The problem is to find the form of the function $\theta(x)$ for which v_l will be a minimum. But the solution of this problem is identical with that of the preceding one. Therefore, in spite of any natural impulse to turn, the proper thing to do is

to apply the brakes and steer straight for the wall. In this case not only will the perpendicular component of the velocity be a minimum, but the parallel component will be zero, that is, the impact with the wall will be the least severe.

The writer wishes to emphasize (especially to casual readers) that *this result applies only to the problem as stated, where the wall is of such extent that one cannot possibly drive around it*, and where considerations involving locations of persons in the car do not enter; for example, if the driver is alone, it may be better to turn to the left before impact. *Highway statistics show that the worst accidents are those involving head-on collisions.* If the wall should stop at the edge of a ditch, it is probably safer to roll over in the ditch than to hit the wall head-on at even moderate speed.⁴

Let us consider a few other simple problems, in which for simplicity we shall be concerned only with circular arcs or straight stops. Suppose we are driving in a highway at a speed such that the minimum stopping distance is D . What is the width W (Fig. 2) of an obstacle that can just be dodged by turning to the side in a circular arc? By a previous result we know that the turning radius is twice the stopping distance. Thus the angle α through which the car turns is 30° , and the angle β subtended by the obstacle is exactly 15° . Note that a wider obstacle at the distance D cannot possibly be dodged, although a collision can be avoided by stopping. Accordingly, when the obstacle subtends an angle of more than 15° , if it can be dodged in a circular turn without slowing down, then the driver can stop. For a car traveling 48 mi/hr, the stopping distance under ideal conditions is about 100 ft *after* the brakes have been applied. Under nonideal but good dry-road conditions, 100 ft may be the stopping distance for a speed of 40 mi/hr. In either case the widest object at the 100-ft stopping distance that could be dodged is 26.8 ft, a big obstacle. But under the condition imposed, the driver, having dodged the obstacle, would then be going full speed at an angle of 30° with respect to the road.

Let us modify this last problem by requiring that, when the car is abreast of the obstacle, it shall be going parallel to the direction of the road (Fig. 3). In this case one finds that the width W

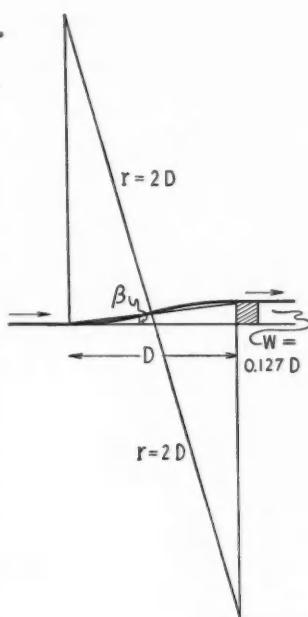


FIG. 3. Path taken by a car dodging an obstacle of width W at a distance D by steering in circular arcs so that the final path is parallel to the original path.

is only 12.7 ft, about the deviation needed to pass a big truck. (While the truck may not be 12.7 ft wide, most drivers want a few feet of clearance, especially when swerving at high speed.) Accordingly, if it is a close question as to whether or not the truck *can* be dodged, it is probable that the car can be stopped straight ahead. In this case, if the truck is moving forward, even though slowly, the advantage of applying the brakes rather than swerving is obvious.

Finally consider the case of two cars approaching each other at right angles but with the same speed. Under these circumstances, most drivers instinctively swerve to the side, each trying to turn at least 45° (Fig. 4). In this case, if the drivers can stop straight ahead without colliding when their speed is v , they can miss each other by turning in a circular arc, even if their speeds exceed v by about 10 percent. This seems to indicate an advantage of turning rather than stopping; but one must remember that, after the cars have missed each other, they will still be going full speed at an angle of 45° with either road. Since their stopping distance at 40 to 48 mi/hr is still 100 ft—the width of a ten-lane highway—the 10-percent advantage seems to be

⁴ Statement made in private communication by an automobile engineer.

overshadowed by the probability that there will be trees or other obstructions along the roadside.

Other similar cases will suggest themselves to the student.

Thus far we have dealt with circular arcs and straight stops. As a practical matter one cannot turn in a circular arc at constant speed without "stepping on the gas" (especially at high speeds), for tire slippage is such as to cause the car to slow down.⁵ If one compensates for the decrease of speed by turning more sharply while slowing down, then the automobile will travel in a spiral path. The same thing will happen if one partially applies the brakes and turns simultaneously so as to use the full value of the friction.⁶

At first thought this slowing down seems to point to the spiral path as an advantageous choice in the original problem. In truth it represents a considerable advantage over the circular arc. While the spiral path may provide a way to overcome a major fraction of the factor of 2 between the circular arc and the straight stop, it has been shown with the aid of the calculus of variations that it cannot overcome all the difference.

It is interesting to plot the path of a particle that is acted upon by a force of constant magnitude at a constant angle γ with the path, so that the tangential and normal components of the acceleration are proportional to $\cos \gamma$ and $\sin \gamma$, respectively. In this case the path can be shown

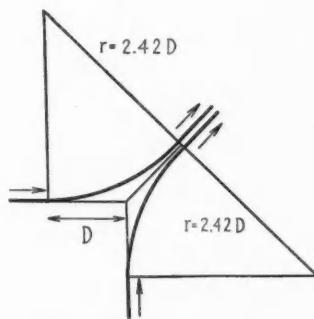


FIG. 4. Paths of two cars approaching an intersection with equal speeds at right angles and then turning in circular arcs.

⁵ For a discussion of tire slippage, see reference 2.

⁶ It should be unnecessary to point out that the full value of the friction cannot be used simultaneously for turning and for stopping. The available force is determined by the coefficient of friction at the speed involved. At least when the wheels are not skidding, the coefficient should be independent of direction (reference 2). The vector sum of the turning force and the retarding force cannot exceed the available friction.

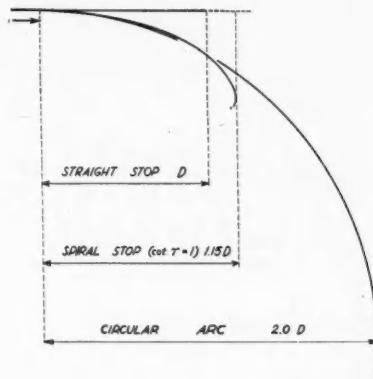


FIG. 5. A comparison of three paths traversed by a particle when acted upon by a force of constant magnitude. For the straight stop, the spiral stop and the circular arc, the forces are, respectively, at 180° , 135° and 90° with respect to the direction of motion.

to be an equiangular spiral,⁷ of the form $\rho = \rho_0 e^{K\phi}$, where $K = \cot \Psi = 2 \cot \gamma$, and Ψ is the constant angle between the radial lines from the pole on the graph and the path.⁸

For instance, let $\cot \gamma = 1$, so that the force acts at 45° with respect to the direction of motion; then, for the spiral path, the stopping distance parallel to the original direction is about 15 percent greater than for the same force with the straight stop (see Fig. 5). Although the "particle" of this problem is hardly an automobile, the equiangular spiral is a curve relatively easy to work with, and is similar in shape to skid marks actually measured on a road surface.

The introduction of the spiral path into the problems of Figs. 2, 3 and 4 will not make any significant changes in the conclusions for the first two, but will change the 10-percent advantage in speed in the problem of Fig. 4 to an advantage of about 20 percent. For instance, on the basis of an equiangular spiral with $\cot \gamma = 1$, if the cars can just stop straight ahead, they can just miss each other if their speeds are 20 percent greater than for the straight stop. A significant fact in this case is that, at the point where the two cars just graze past each other, they will have lost about 45

⁷ For instance, see S. L. Loney, *Dynamics of a particle and of rigid bodies* (Cambridge Univ. Press, 1913), p. 99.

⁸ The factor of 2 in the expression $\cot \Psi = 2 \cot \gamma$ is associated with the factor of 2 between the right-hand members of $F_t = \frac{1}{2}mv_0^2/l$ and $F_r = mv_0^2/l$ previously referred to, and cannot be neglected.

percent of their original speed. The important thing to notice then, is that one should apply the brakes and turn, but that one should not do either one so strongly as to cause the car to skid. On the basis of this calculation ($\cot \gamma = 1$), we assume that 0.707 of the available force will be used for braking and that (at right angles) 0.707 of the force will be used for turning. In other words, the retarding force (largely provided by the brakes) should be about 70 percent of the maximum.

Returning to the original problem (Fig. 1) we shall consider a few points that we have heretofore neglected. Only for one particular value of the coefficient of friction is it possible to adjust four-wheel brakes so that the maximum braking force can be exerted by all four wheels at all speeds.⁹ As a practical matter, on the usual road surfaces about 80–90 percent of the maximum theoretical force can be applied, the actual value depending on the ratio of braking power between the front and rear wheels. Accordingly measured stopping distances are perhaps 10–20 percent longer than those calculated from the coefficient of friction applicable at the speeds involved. This argument tends to support turning in a path of considerable curvature.

On the other hand, few cars are so built that, in negotiating turns they make full use of the coefficient of friction at both the front and rear wheels.¹⁰ On some cars the measured coefficients of friction required to prevent skidding for the rear wheels were found to be as much as two or three times larger than the coefficients required

⁹ The maximum theoretical deceleration depends on the coefficient of friction and the weight of the car. Cars cannot ordinarily be built so that the front and rear wheels exert an equal force on the car in braking at all values of deceleration because the effective weight transfer from the rear to the front wheels depends on the deceleration (see reference 2, pp. 75 ff.). The stopping distances on dry roads will be shorter when the front wheels rather than the rear wheels exert the larger force in braking, but on wet or slippery roads (when deceleration must be small) the front wheels may then skid too easily, thus reducing the effectiveness of the steering mechanism.

¹⁰ In turning, since the wheels are not skidding sideways, we are concerned with static friction. Few textbooks distinguish between the force of static friction f and the maximum available force of static friction F_{\max} . It is only F_{\max} that is equal to the coefficient of friction multiplied by the normal force; f may be anything less than F_{\max} . For a good discussion see Duff, *Physics* (Blakiston, ed. 8, 1937), pp. 88–90. For instance, although the coefficient of friction (as properly defined) may be finite, the force of friction for a block resting on a level table with no applied horizontal forces is zero.

for the front wheels.¹¹ These cases may be extreme, but they show that if the full value of the available friction is being used by one set of wheels, then the other may be using considerably less than the available friction, and, of course, in that case one cannot turn so sharply as would otherwise be possible. This argument tends to support the straight stop.

Another point is that, even disregarding reaction time, it is impossible to turn instantly from a straight path into a curve of finite radius of curvature. From 1 to 2 sec probably will be needed to develop the full value of the centripetal force, especially if the steering wheel must be turned very far.¹² The brakes can be applied to their full effect in much less time. This argument also tends to support the straight stop.

So far we have not explicitly considered the question of skidding. As is well known, the coefficient of sliding friction is less than that of nonsliding friction.¹³ It would therefore seem on first thought that the component of momentum of the car perpendicular to the wall (in the original problem) would be reduced less if the car were thrown into any sort of skid than it would be if the wheels were allowed to roll with the brakes applied as much as possible without sliding the wheels. In actual practice, however, the brake power ratio between the front and rear wheels may have been so designed that this conclusion is not valid.⁹

* Strictly speaking one cannot steer a skidding car; the car simply slides. The solution of the original problem was to apply the brakes and steer straight ahead. If the brakes are applied too hard—to the point of skidding all the wheels—it is unnecessary to try to steer, for the car will slide more or less straight ahead anyway. It is doubtful if the ordinary driver is ever justified in attempting to skid; but, should a skid occur, it is much easier to recover from a skid straight ahead, caused by excess braking, than from one that develops while turning or swerving.

Racing drivers habitually skid around curves on purpose. However, this does not provide an argument against the conclusions reached in this paper, because in racing the main idea is to get around the turn as rapidly as possible

¹¹ Reference 2, p. 123, point 27.

¹² R. A. Moyer, private communication.

¹³ See reference 2, pp. 56 ff.

without losing speed (and definitely without stopping). Our concern is with a different general problem—that of missing an obstruction by the greatest distance—where in some cases stopping is the best procedure.

In the simple cases considered here, the results indicate advantages of stopping rather than turning; at least in the problem of Fig. 4 they indicate that the brakes should be put on to a major degree. This does not imply that stopping is *always* better than turning. However, in borderline cases where the theory indicates no significant advantage of one or the other choice, the fact that a driver may go into a dangerous skid if he swerves is probably an argument in favor of putting on the brakes.¹⁴ Even in cases where a spiral path may be indicated, it should be remembered that, unless one steers in the proper spiral path, either all the available friction may not be used or else the car may go into a dangerous skid. Steering in an equiangular spiral may present some difficulty to a mathematics student, let alone a man faced with an impending automobile collision! Putting on the brakes is less complicated than this, and often more effective.

As a practical matter these results are probably less applicable in cases where an accident is almost certain to occur, for example, for a car speed of 40–48 mi/hr and a 12.7-ft obstacle 100 ft away (Fig. 3), than in cases where the driver has a fraction of a second to make up his mind what to do, for example, when the same obstacle is 200 ft away. In the latter case, if the driver is to be allowed the usual three quarters of a second (44–45 ft) to move his arms or his feet *after* he decides what to do, he will have only a fraction of a second to make up his mind *what* to do. Under these circumstances he may either stop or swerve, provided he knows there are no turns in the road, no crests of hills and no cars approaching! Otherwise he should apply the brakes.

In all but rare cases it is easy enough to show that if an object can be dodged at high speed, it can still be dodged if one first steers straight with the brakes full on, and then turns. Thus drivers

¹⁴ R. A. Moyer points out in a private communication that average coefficients of friction calculated from measured straight stopping distances may be as large as 0.7, whereas those calculated from measured circular turns on similar surfaces have been only 0.5. Thus for an automobile rather than for a "particle," *more* friction is available for stopping than for turning.

should be urged to develop the habit of *first putting on the brakes* while steering straight, perhaps later modifying their choice *after* they have slowed down.¹⁵

Many fast drivers vaguely realize that they may not always be able to stop if trouble develops, but they *hope* they may be able to steer out of a difficulty when it arises. Perhaps the most significant feature of this paper is in showing that *frequently turning offers less chance for safety than does stopping*. Therefore, drivers should always be prepared to stop.

The proof of the basic conclusion in this paper—that in the original problem, *stopping with the brakes full on without turning* is the proper procedure—lies of course in tests actually performed. Unfortunately it is impossible at this time to disclose specifically the results of these tests, or by whom they were made.

In general, when the brakes were applied full on (in some cases skidding some of the wheels) and the car was steered straight ahead, the stopping distances (always measured parallel to the direction of the original velocity) were found to be shorter by some 5 to 15 percent than when the attempt was made to turn *and* stop. In the case of turning as sharply as possible without applying the brakes, the distance at which the car had turned 90° was some 30 to 50 percent greater than for the straight stop. Needless to say, in some cases when the brakes were put on only *part way*, the advantage of the straight stop over the turn-and-stop method disappeared.

Casual readers should be cautioned against drawing conclusions about other situations in which the present results may not apply. Moreover, any driver who modifies his driving habits along the lines discussed in this paper should use considerable discretion until he becomes familiar with the way the ideas work out under actual driving conditions.

The writer wishes to express his appreciation for helpful discussions with Professor Winston Hole, of Michigan State Normal College, and Professor R. A. Moyer, of Iowa State College.

¹⁵ Some traffic schools recommend stopping straight ahead on the basis that drivers in other lanes are not jeopardized; the straight stop is here recommended because it also is the shortest stop.

Henry Crew

Recipient of the 1941 Oersted Medal
for Notable Contributions to the
Teaching of Physics



The American Association of Physics Teachers has made the sixth of its annual awards for notable contributions to the teaching of physics to Professor Henry Crew, of Northwestern University. The addresses of recommendation and presentation were made by Professor Richard M. Sutton and Professor A. G. Worthing in a ceremony held in Palmer Laboratory, Princeton University, on December 30, 1941, during the eleventh annual meeting of the Association.

ADDRESS OF RECOMMENDATION BY PROFESSOR RICHARD M. SUTTON

GALILEO has often been called the "Father of Physics," and those of us who instruct in the field that he sired acknowledge increasingly our debt to the great investigator and experimenter of Pisa, Padua, and Florence. Today, in presenting the Oersted Medal of this Association, we seek to do honor to the "Father of Galileo"—PROFESSOR HENRY CREW, of Northwestern University, who through his translation into English of GALILEO's *Dialogues concerning two new sciences*¹ has done much to bring the works of that doughty pioneer of physics within the reach of us all, and whose long career as a teacher and writer has been an inspiration to many. This man who has contributed steadily to our field of learning over a period of more than 50 years has left many guideposts along his way that "he who runs may read," and some of these have become permanent landmarks in the field. PROFESSOR CREW's writings have

run the gamut from high school textbooks and laboratory manuals² to the translations of medieval Latin documents on optics, and they show a depth of scholarship coupled with elegance of style that bespeak the quality of broad classical training that is all too rare in our contemporary streamlined education.

It is a fitting coincidence that we are meeting today in Princeton, New Jersey, at the University where, 63 years ago, HENRY CREW entered the freshman class as a youth of 19, where he was a classmate of PRESIDENT HIBBEN's and a contemporary of WOODROW WILSON's. HENRY CREW was born in Richmond, Ohio, in 1859. His father died when he was 11, and his mother took the boy, Henry, and his two

¹ *Dialogues concerning two new sciences*, by Galileo Galilei (1632), translated from Italian and Latin by Henry Crew and Alfonso de Salvio (Macmillan, 1914).

² The textbooks and manuals are: *Elements of physics for use in high school* (Macmillan, 1899, 1913); *General physics for colleges* (Macmillan, 1908, 1921, 1927); *Laboratory manual for use in high schools*, with R. R. Tatnall (Macmillan, 1902, 1911); *Mechanics for students of physics and engineering* (Macmillan, 1908), and with K. K. Smith (Macmillan, 1930); *The wave theory of light—An historical monograph*—(American Book Co., 1900).

younger sisters to Wilmington, Ohio, for their schooling. From his preparatory work at Wilmington, he entered Princeton University in 1878 and here cultivated an absorbing interest in physics. After graduation from Princeton in 1882, he spent two valuable years in Germany on a fellowship, studying under HELMHOLTZ, KIRCHHOFF and KAYSER, and then returned to this country to complete his graduate work at Johns Hopkins University under HENRY ROWLAND. It would have been astonishing if he had emerged from such training as anything other than a spectroscopist!

In 1888, before many in this room had seen the light of day, and when most of the rest knew physics only as something to be taken internally, HENRY CREW accepted his first teaching position as instructor at Haverford College, where he remained for three years, long enough to marry a member of the first graduating class from Bryn Mawr College and to establish a home. The house that he built on the Haverford campus still stands, and since I now occupy it, I have had ample opportunity to judge of his taste in architecture. However, the Oersted Medal is awarded to PROFESSOR CREW for notable contributions to the teaching of *physics*, and the architectural sins of the late Victorian period should be forgiven because, in 1890, "household physics" was still unknown.

After a year as astronomer at Lick Observatory, PROFESSOR CREW became head of the department of physics at Northwestern University, a position which he held until 1925 when he relinquished his administrative duties to PROFESSOR SPENCE. He retired from active teaching in 1930 after nearly 40 years at that university. During that time he had done his writing, had been an associate editor of the *Astrophysical Journal*, President of the American Physical Society (1909), President of the American Association of University Professors (1929) and President of the History of Science Association (1930), as well as an active promoter of science groups in Illinois and a worthy citizen of Evanston. For three years following his retirement, he served as Chief of the Division of Basic Sciences at the Century of Progress Exposition. In recent years he has devoted himself chiefly to golf, automobile driving—

learned at the age of 74—and the management of a new automatic furnace. These are, of course, purely extra curricular activities, for he finds continued interest in applying his love of the classics to the unraveling of medieval Latin. His recent translation of the *Photismi de lumine* of MAUROLYCUS³ is already well known, and he is now at work on a translation of *The Common Optics of John Peckham, Archbishop of Canterbury*. It seems that English prelates of the thirteenth century had nothing better to do than to write books on optics!

Most of this factual recital is common knowledge, sufficient to leave no doubt about the productive scholarship and long, devoted service of PROFESSOR CREW, and of the breadth of his contributions to physics. But it would be unfortunate to present this man to you only as the gaunt framework of reference for certain biographical facts without disclosing also the warmth of personality and the strength of character that have endeared him to his students and colleagues. One student, now professor of physics in another institution, says:

I think that the most unusual and outstanding thing about him was his extraordinary care to see that the student working with him never missed any help or attention which he could give. . . . I think I have never known another teacher of like standing and like responsibilities who took his obligations to students so much to heart.

This same sentiment has echoed clearly from other quarters as well.

PROFESSOR CREW always expected a high standard of performance from his students and many have expressed their appreciation of the rigorous training received under him. His best students may remember him best, but he, himself, was always deeply concerned over the poorer students as well, and would suffer for them as if, by some unusual effort of his own, he could expiate their shortcomings. Those who have been closely associated with him tell of his endeavor to encourage independence of thought and clear judgment on the part of the students. He has always put the learner upon his own responsibility, aiding the learning process by asking penetrating questions, and only as a

³ The *Photismi de lumine* of Maurolycus, tr. from Latin (Macmillan, 1940).

last resort, by telling the student directly. Our newly elected president, PROFESSOR KNOWLTON, himself a former student of PROFESSOR CREW'S, relates how on one occasion he assisted a young lady with the wiring of a potentiometer. About that time PROFESSOR CREW passed by and remarked to the young lady, "Miss Jones, isn't it good of Mr. Knowlton to do that for you so that you will never learn how to do it for yourself?"

In his lectures he was a master of exposition and constantly tried to present physics as a living and growing subject, filled with human interest and not dissociated from the men who have helped to develop it. His basic philosophy in this regard seems to have been that the facts and laws of physics take on another dimension of interest when they are associated with the men who discovered and conceived them. His effort to instil this kind of interest into the science is perhaps best recorded in his well-known book, *The rise of modern physics*.⁴

In a paper read before the American Historical Association in Cleveland several years ago,⁵ PROFESSOR CREW made a strong plea for more attention to the history of science in the college curriculum. It would be valuable, if time permitted, to read his paper to you in full, but we shall have to be content with a short extract from it which brings out a portion of his thought:

Is science in America to be forever presented as a set of abstract principles, a set of generalizations, derived, it may be, in the first instance, from experiment or observation, but now formulated in the most impersonal way possible? Or is science to be treated as a distinctly human achievement?

Are not experimental results simply the facts which human beings have deciphered from the scroll of nature? Are the laws of physics and chemistry anything more than human expressions, adapted to finite human capacities, and accepted because of their convenience in human conversation? Is a theory in astronomy anything more than the "present policy"—to use Sir J. J. Thomson's phrase—proposed by some human observer for convenience in making prediction for later human observers? Are the bodies of plants and animals anything more to the student of biology than the source books from which some human investigator has unraveled the laws of development and hygiene—the laws of birth, growth, death and

⁴ *The rise of modern physics* (Williams & Wilkins, 1928, 1935).

⁵ "The problem of the history of science in the college curriculum," *Sci. Mo.* 10, 475 (1920).

disease? Do the various extensions of physical and natural laws to include new observations mean anything more than the fact that present investigators stand upon the shoulders of their fathers and thus acquire a wider and deeper vision?

PROFESSOR CREW'S laboratory and home are filled with the pictures of the men who have made physics. On one occasion, a picture of RUTHERFORD was taken by some acquisitive person, and when the loss was discovered, PROFESSOR CREW wrote the following letter to the student newspaper of Northwestern University.⁶

January 3, 1927

To the young man who has the portrait of Sir Ernest Rutherford, recently removed from its place in the hall of the Physical Laboratory:

My dear friend:

It was a mere prank, I am sure, for no one who admires, as you do, the fine face of Sir Ernest Rutherford, beaming with honesty and straightforwardness would want in his collection a portrait not rightly acquired. You know, quite as well as I do, the spirit in which the keen blue eyes of this fearless knight—a man who has spent his entire life in hot pursuit of truth—would be constantly looking down upon you.

If, therefore, at your first opportunity, you will quietly restore this great Englishman to his proper place, in the good company of his teacher, Sir J. J. Thomson, and of the genial Ampère, you and I will each be happier, and what's more, you may realize anew, and for the rest of your life, the meaning of that freedom which comes with the truth and which it is the aim of every university to confer.

Furthermore, any feeling of sorrow on the part of Northwestern University, a branch of the nation's service based on integrity and loyalty, will be reversed into pride at your action. Please believe me.

Your friend,
HENRY CREW.

In view of the failure of the picture to make its reappearance, one can only conclude either that the culprit was not a student at Northwestern, or that he could not read!

PROFESSOR SPENCE, now chairman of the department of physics at that institution, related to me recently how he happened to enter physics. As an undergraduate, SPENCE was greatly interested in chemistry, but he found no encouragement in his efforts to start independent work in that field. However, when he visited

⁶ *Daily Northwestern*, Jan. 7, 1927.

PROFESSOR CREW and expressed interest in carrying out an individual piece of experimentation in physics and asked when he might start, PROFESSOR CREW turned to him and said, "Take off your coat!"

Another colleague of 20 years acquaintance speaks of his admiration for the warmth and sincerity of the man, and remarks, "It is im-

possible to walk around the block with CREW without feeling that it was worth while."

PRESIDENT WORTHING, I take pleasure in presenting to you as recipient of the Oersted Medal of this association, a man who is a master of exposition, both written and oral, a sound physicist, a devoted teacher, a gentleman and a scholar: PROFESSOR HENRY CREW.

PRESENTATION OF AWARD BY PROFESSOR A. G. WORTHING

Through the generosity of an anonymous doner, the American Association of Physics Teachers has been enabled to make annual awards for notable contributions to the teaching of physics. Except for the first of the series, which was made posthumously in 1936 to WILLIAM SUDDARS FRANKLIN, the award has consisted of a medal of bronze and a certificate, both of which have been presented at the annual Christmas meeting. The medal known as the *Oersted Medal*, on the obverse shows HANS CHRISTIAN OERSTED, the teacher, before an audience at the moment of his great discovery of electromagnetism in 1820. On the reverse it bears the words, "For Notable Contributions to the Teaching of Physics, Awarded to," followed by the name of the recipient. The recipients of

the medal have been EDWIN HERBERT HALL (1937), ALEXANDER WILMER DUFF (1938), BENJAMIN HARRISON BROWN (1939), and ROBERT ANDREWS MILLIKAN (1940). Today we add another to this brief list of those to whom we extend our highest honor.

PROFESSOR HENRY CREW, "man who is a master of exposition, both written and oral, a sound physicist, a devoted teacher, a gentleman and a scholar," I have the great pleasure and privilege to present to you, as authorized by its executive committee, the 1941 Oersted Award and certificate of the American Association of Physics Teachers as a token of its regard and indebtedness. May we look forward to your continued inspiration and leadership.

ADDRESS OF ACCEPTANCE BY PROFESSOR HENRY CREW

MEN of advanced age have to be careful not to take themselves too seriously. I am asking you, therefore, to join me in properly discounting some of the things that the preceding speakers have said.

It has been a privilege of the first order for me to be present at this meeting and to meet so many long-time friends. I am glad also of an opportunity to express to the members of this association my sincere and heart-felt thanks for the distinct honor which they have just conferred upon me.

It is a pleasure also to be associated even in a slight degree with the name of that eminent teacher, H. C. OERSTED, the man who erected one of the two solid pillars upon which MAXWELL built his exquisite theory of electromagnetism. The mention of OERSTED's name always brings to mind an experience I had when last in Europe. It was in the autumn of 1930, when I had the pleasure of spending several days in Copenhagen.

As a student of physics, I was naturally curious about the haunts of OERSTED, and at once attempted to locate the lecture-room in which he first demonstrated the phenomenon of electromagnetism to a group of students. My first inquiry was made of that omniscient individual who stands—or used to stand—near the entry of every first-class hotel in Europe. In this case, it was the "porter" of the Hotel d'Angleterre, where I was stopping. To my surprise, he had never heard of OERSTED. My next inquiry was made at a large bookstore, where one generally finds an abundance of academic information; but here again OERSTED was a prophet without honor in his own country. On the afternoon of that same day, however, I had to drive into the country, some four miles from the city, to visit WALDEMAR POULSEN, the engineer who had invented the recording telephone, an instrument which I was inviting him to exhibit at the Century of Progress

Exposition. Among other things, I asked POULSEN if he knew where OERSTED had performed his famous experiment. "Certainly!" he replied, "I'll be glad to show you the place;" and he got into my waiting taxicab and drove directly to the large new business block that now houses the Telephone Company of Denmark. Here, at the rear end of a long entrance hall or corridor, stood a great block of solid granite, upon which was placed a marble bust of OERSTED. On the front face of this granite block was chiseled the following inscription:

Here on the 21st of July 1820, Hans Christian Oersted, Professor in the University of Copenhagen, proved that the electric current directs the magnetic needle across the direction of the current. He thus laid the foundation of electromagnetic telephony.

Well, I am glad not only to be associated, even in a nominal way, with such a teacher as OERSTED, but I am also happy to be back in this neck of the woods where I first met several really great teachers. One of them—the only one living of all my teachers—I had the pleasure of visiting this morning. It was from this one surviving teacher that I first learned the dovetailedness of the sciences; for, in his presentation of geology, he employed biology, chemistry, physics and astronomy without the slightest hesitation. I refer, of course, to PROFESSOR W. B. SCOTT. But in addition to this he taught us the importance of fidelity to the facts. Spare no pains to get the facts, and then remain loyal to them. In this respect he went MARK TWAIN one better. You know the story of RUDYARD KIPLING's visit to MARK TWAIN. Strange as it may seem, they fell to talking about books. "In this matter of authorship, the important thing," drawled MARK, "is to get the facts. First get your facts; and then you can distort them at your leisure." No one, I am sure, appreciated more keenly than PROFESSOR SCOTT this liberty which the *littérateur* enjoys.

Since we are gathered here not only as fellow students but also as fellow teachers of physics, I should like to say just one word about our goal, about what a teacher of physics may fairly aim at. I do not know how it is with you; but, as for me, my ideals have always been derived mainly from certain concrete examples; from warm-blooded men and women.

I have in mind a well-known remark of Mr. GILMAN that the kind of man he wanted at the

head of each department in Johns Hopkins University, was "a student who can also teach." It is only the student-teacher who can at once keep abreast of his subject and, at the same time, be in sympathy with the men in his classes. It is only the student-teacher who can meet the oncoming generation with becoming modesty and also with accurate scholarship. One is here reminded of those two lines from *Faust* with which BOLTZMANN prefaces his lectures upon MAXWELL's electromagnetic theory:

So soll ich denn mit saurem Schweiss
Euch lehren, was ich selbst nicht weiss.

Shall I attempt to explain to you young gentlemen
something which I myself do not understand.

Yes, certainly! I think we would all answer; for that is the best way to clear up all the difficulties of both teacher and taught. I have always had great sympathy with the Cambridge don, who, when asked for some bit of information more or less along his line, replied "Sorry! But that is a subject I know nothing about. In fact, I have never even lectured upon it."

Here was a man who delighted to study a subject along with his students. His pleasure is that of the young child with the building blocks. The little fellow lays down some big blocks for a foundation; others go to make up the walls; next comes the roof; finally a steeple; then bang goes the whole structure to the floor. The fun was in the building. The fun of teaching is in building, along with the student; building both scholarship and character.

In my home town, Chicago, some students were recently asked to define the word *date*. My sympathy is with the girl who handed in this reply: "A date is a cooperative form of recreation." The student who gets from his teacher sympathy and cooperation need not worry much about streamlined laboratory tables or chromium-plated apparatus. PROFESSOR SUTTON has already demonstrated to this group the charm and effectiveness of simple apparatus.

I am wondering whether the best teaching in physics is not a *cooperative form of recreation, supervised by a student who can also teach*. Only in this way is education likely to become a transaction between two warm-blooded individuals, and thus a part of life rather than a mere preparation for life.

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Poiseuille's Observations on Blood Flow Lead to a Law in Hydrodynamics

J. F. HERRICK

Division of Experimental Medicine, Mayo Foundation, Rochester, Minnesota

IN recent years advances in the biological sciences have been largely dependent on the application of the physical sciences. The departments of biochemistry and biophysics found in practically all colleges and universities today give evidence of the fund of knowledge that has already been obtained and is being obtained by the fruitful application of the methods of chemistry and physics to the biological field.¹

The important role which the physical sciences have played in the progress of the biological sciences has eclipsed, more or less, the contributions which biologists have made to the physical sciences. Some of these contributions have become such an integral part of the physical sciences that their origin seems to have been forgotten. An outstanding example of such a contribution is that by Jean Léonard Marie Poiseuille² (1799–1869). About 100 years ago Poiseuille brought a fundamental law to that division of physics known as hydrodynamics—which is a branch of rheology, according to more recent terminology. This law resulted indirectly from his observations on the capillary circulation of certain animals. Most physicists, chemists and mathematicians associate the name of Poiseuille with the phenomenon of viscosity because the cgs absolute unit for the viscosity

coefficient has been named the *poise* in his honor. Few know the story leading up to the discovery of the law which bears his name. This law had more fundamental significance than Poiseuille himself realized. It established an excellent experimental method for the measurement of the viscosity coefficients of liquids. The underlying principle of this method is in use today. Since Poiseuille's law was based entirely on experiment, it was purely empirical. However, the law can be obtained theoretically. Those who are familiar with only the theoretical development are generally surprised to learn that the law was originally determined experimentally—and still more surprised to know that Poiseuille got his idea from studying the character of the flow of blood in the capillaries of certain animals.³

Poiseuille was, at heart, a physicist. At the age of eighteen years he entered the École Polytechnique in Paris, where he received good training in physics and mathematics. When this school closed its doors, he entered the medical school. His thesis for the M.D. degree was a study of the strength of the aortic heart. After receiving his degree he became free teacher of medical physics. It is said that he always devoted some time out of each day to mathematics and physics.

The epoch-making experiments of Poiseuille which established the law bearing his name are published in a journal not readily accessible. However, Brillouin⁴ has described them in considerable detail and Bingham^{5(a)} has outlined them more briefly. The appendix of Bingham's book contains Poiseuille's original data. Recently Bingham^{5(b)} has edited a monograph that contains a translation by Winslow H. Herschel

¹ One of the most recent interesting educational developments pertaining to the training of future biophysicists and biochemists is the establishment of a new type of department called *biological engineering* at the Massachusetts Institute of Technology; see K. T. Compton, "Possibilities in biological engineering," Ann. Int. Med. 12, 867–875 (1938).

² J. L. M. Poiseuille: (a) "Recherches sur les causes du mouvement du sang dans les vaisseaux capillaires," Compt. rend. Acad. d. sc. 1, 554–560 (1835); (b) "Recherches sur les causes du mouvement du sang dans les vaisseaux capillaires," Mém. prés. Acad. d. sc. de l'Inst. de France, Par. 7, 105–175 (1841); (c) "Recherches expérimentales sur le mouvement des liquides dans les tubes de très petits diamètres," Compt. rend. Acad. d. sc. 11, 961–967, 1041–1048 (1840); 12, 112–115 (1841); 15, 1167–1187 (1842); (d) "Experimentelle Untersuchungen über die Bewegung der Flüssigkeiten in Rohren von sehr kleinen Durchmessern" [Bericht einer aus den H. H. Arago, Babinet, Piobert und Regnault gebildeten Kommission über diese Abhandlung], Ann. de chim. et de Phys. 7, (3) 7, 50 (1843)]; Ann. d. Phys. u. Chem. 58, 424–448 (1843); (e) "Recherches expérimentales sur le mouvement des liquides dans les tubes de très petits diamètres," Paris, Mém. Savans Étrange 9, 433–544 (1846).

³ When one of the leading hydraulic engineers in our country—R. E. Horton—learned that I was reading the original publications of Poiseuille with the idea of publishing a note concerning them, he wrote: ". . . I hope I may induce you to say something that will tend to restore to Poiseuille the credit to which he is entitled."

⁴ M. Brillouin, *Leçons sur la viscosité des liquides et des gaz* (Gauthier-Villars, Paris, 1907), Vol. II.

⁵ E. C. Bingham: (a) *Fluidity and plasticity* (McGraw-Hill, 1922); (b) *Rheological memoirs* (Lancaster Press), vol. 1, No. 1.

of Poiseuille's epoch-making paper,^{2(c)} "Recherches expérimentales sur le mouvement des liquides dans les tubes de très petits diamètres." Barr⁶ gave a detailed description of Poiseuille's viscometer and paid a nice tribute to him when he wrote about his experimental work thus: "It forms one of the classics of experimental science and is frequently quoted as a model of careful analysis of sources of error and painstaking investigation of the effects of separate variables."

Poiseuille's experiments on the flow of liquids in glass tubes were so simple and the results so surprising that a committee was appointed to investigate the research. The experiments were repeated carefully and no error could be found. The experiments can be repeated today with the same results. Poiseuille made his experiments for the ages. What more can be said of an experimental investigator?

Poiseuille's results were surprising because the new law he derived was not in agreement with the laws of flow of liquids previously developed except for the law developed by Hagen⁷ one year previously. Girard,⁸ an outstanding physicist at the time, had found a more complex relation between volume flow, pressure and the dimensions of the tube. Navier,⁹ and later Stokes,¹⁰ developed a law for the flow of liquids in tubes from a purely theoretical point of view.

⁶ Guy Barr, *A monograph of viscometry* (Oxford Univ. Press, 1931).

⁷ G. H. L. Hagen, "Ueber die Bewegung des Wassers in engen cylindrischen Röhren," Ann. d. Phys. u. Chem. **46**, 423-442 (1839).

⁸ P. S. Girard: (a) "Mémoire sur le mouvement des fluides dans les tubes capillaires, et l'influence de la température sur ce mouvement," Paris, Mém. de l'Inst. 249-380 (1813-1815); (b) "Mémoire sur l'écoulement linéaire de diverses substances liquides par des tubes capillaires de verre," Paris, Mém. Acad. Sci. **1**, 187-259 (1816); (c) "Mémoire sur l'écoulement linéaire de diverses substances liquides par des tubes capillaires de verre," Ann. de chim. **4**, 146-164 (1817); (d) "Mémoire sur l'écoulement de l'éther et de quelques autres fluides par des tubes capillaires de verre," Paris, Mém. Acad. Sci. **1**, 260-274 (1816); Ann. de chim. **6**, 225-238, 334-336 (1817).

⁹ C. L. M. H. Navier: (a) "Sur les lois des mouvements des fluides en ayant égard à l'adhésion des molécules," Ann. de chim. et phys. **19**, 244-260 (1821); (b) "Mémoire sur les lois du mouvement des fluides," Paris, Mém. Acad. Sci., **6**, 389-440 (1823); **9**, 311-378 (1830).

¹⁰ G. G. Stokes: (a) "On some cases of fluid motion," Trans. Camb. Phil. Soc. **8**, 105-137 (1849); "On the theories of the internal friction of fluids in motion and of the equilibrium and motion of elastic solids," Trans. Camb. Phil. Soc. **8**, 287-319 (1849); (b) *Mathematical and physical papers* (Cambridge, 1880), vol. 1, p. 75.

Navier's equation did not agree with that of Poiseuille. The beauty of Poiseuille's experiments and their results was their simplicity. At this point I am prompted to introduce a quotation from a review by Swann¹¹ of Eve's biography of Lord Rutherford. In reference to the work of Rutherford the statement is made:

Most worthy of all is the extreme simplicity and directness of his experimental methods. Some observers seem to grow happier as their apparatus becomes more complex.

Poiseuille's complete data are available for any one who wishes to study them. Many competent critics have done so. Knibbs's¹² paper on "The history, theory and determination of the viscosity of water by the efflux method" is an example of a way in which these data have been used.

It is seldom that any investigation can be regarded as final. Despite the fact that the unit for the viscosity coefficient is named the poise, Poiseuille himself did not introduce this coefficient into his formula. The constant factor in his law included this coefficient but it was not introduced explicitly until 1860 by Hagenbach¹³ and also by Jacobson.¹⁴ Navier, however, used this coefficient in his theoretical treatment although it was not definitely named. Today, other corrections and modifications are being made in Poiseuille's law according to the demands of the problem to which it is being applied. In a recent paper Hersey and Snyder¹⁵ wrote:

Departure of Eq. (3) from the usual form of Poiseuille's law should not, however, be taken to indicate any failure of Poiseuille's law in a physical sense. In

¹¹ W. F. G. Swann, "The life and letters of Lord Rutherford," Science **91**, 46-48 (Jan. 12, 1940).

¹² G. H. Knibbs, "The history, theory and determination of the viscosity of water by the efflux method," N. S. Wales Roy. Soc. J. **29**, 77-146 (1895); "Note on recent determinations of the viscosity of water by the efflux method," N. S. Wales Roy. Soc. J. **30**, 186-193 (1897).

¹³ Eduard Hagenbach, "Ueber die Bestimmung der Zähigkeit einer Flüssigkeit durch den Ausfluss aus Röhren," Ann. der Physik **109**, 385-426 (1860).

¹⁴ Heinrich Jacobson, Arch. f. Anat., Physiol. H. Wissensch. Med.: "Zur Einleitung in die Hämodynamik," 305-328 (1861); "Beiträge zur Hämodynamik," 80-112 (1860); "Beiträge zur Hämodynamik," 683-702 (1862); "Ueber die Blutbewegung in den Venen," 224-242 (1867).

¹⁵ M. D. Hersey and G. H. S. Snyder, "High-pressure capillary flow. Theory of nonuniform viscosity; illustrated by experimental data," J. Rheology **3**, 298-300 (1932). Eqs. (1) and (3), mentioned in the quotation, are equations that appear in the paper.

fact, the proof of Eq. (1) and hence of Eq. (3) is obtained by integrating the pressure drop along successive elements of the capillary, treating the viscosity as a constant in any one element and computing the flow through each element on the basis of Poiseuille's law.

It was the nature of the flow of blood in the capillary vessels of certain animals that challenged Poiseuille to begin the study of the laws governing the flow. Obviously, the way to begin such an investigation is to set up an artificial schema—an analog—whereby the supposed controlling factors may be studied separately. After developing his law, Poiseuille did not make a thorough investigation concerning its validity in the flow of blood. This problem was left to his followers.

Interestingly enough, physiologists have found that Poiseuille's law is not valid for the flow of blood. This is not surprising for several reasons, one being the nature of the liquid. Poiseuille's experiments were confined to homogeneous liquids flowing uniformly in small capillary glass tubes. Blood is a suspensoid and its viscosity is a function of the number and size of the corpuscular elements present. Poiseuille set out to place the flow of blood, as he observed it in capillaries, on a rational basis and he arrived at a deduction that does not apply rigorously to blood. However, this does not, in any way, detract from the contribution which he made to the science of hydrodynamics.

The experiments which stimulated Poiseuille to study the flow of water in glass capillary tubes are seldom mentioned. I have spent years in the measurement of blood flow and I was therefore keenly interested in the fact that the study of the circulation of the blood prompted Poiseuille's epoch-making experiments. For this reason his original papers on the flow of blood were consulted. Poiseuille's observations on blood were made as painstakingly and completely as his famous experiments on the flow of water.

From the time that Malpighi¹⁶ discovered the capillaries to the time of Poiseuille, physiologists had observed the capillary circulation and had postulated hypotheses for the mechanism of movements as viewed through the microscope.

Only one term, "globule," was used when referring to the various cells in the circulating blood. These globules could be seen to assume various velocities under apparently the same conditions. If one would focus the attention on any two globules, he might observe that they seemed to be moving along with the same velocity because the two remained the same distance apart. Suddenly the one ahead would appear to slow down and the second globule would approach it. At other times the one in the rear would slow down and thus the distance between them would increase. At times the globules would appear to be stationary. Most curious of all was the rotary motion of the globules. Some globules would be undergoing rotation alone, some would be only in translation and others would have both types of motion simultaneously.

These observations led physiologists to make the assumption that the globules had the power of spontaneous activity similar to infusoria. Such a hypothesis did not require a control by the heart. In fact, the flow of blood in the capillaries was considered to be independent of the action of the heart. Physiologists at that time accepted the fact that the heart controls the flow of blood in arteries and veins, but they postulated two activating agents for the flow in capillaries: (1) the change in caliber of the capillaries due to their contractile power—a sort of sucking force on the part of these capillaries (the latter is according to the diction of a century ago); (2) spontaneous activity of the globules themselves which controlled not only their own motion, but that of the blood as well. Haller,¹⁷ an eminent physiologist of the eighteenth century, believed in an attraction of the globules for one another. Poiseuille limited most of his observations to the capillaries in the web of the frog's foot, in the tadpoles of the salamander and the frog and in the mesentery of the frog, salamander, young rats and young mice.

Before undertaking his studies on the move-

¹⁶ Albertus Haller: (a) *Deux mémoires sur le mouvement du sang, et sur les effets de la saignée; fondés sur des expériences faites sur des animaux* (Bousquet, Lausanne, 1756); (b) *A dissertation on the motion of the blood, and on the effects of bleeding, verified by experiments made on living animals. To which are added, observations on the heart, proving that irritability is the primary cause of its motion.* Tr. by a physician. (J. Whiston, London, 1757).

¹⁶ M. Malpighi, *De pulmonibus* (1661).

ments of the globules, Poiseuille performed experiments which proved clearly that the flow of blood in the capillaries is controlled by the heart. He placed ligatures on the femoral artery of the frog when observing the capillaries in the web of the foot. This caused a stoppage of the flow. When he removed the ligature, the flow again took place, the motion originating in the axial stream. After a thorough study of the effect of ligating the artery, Poiseuille proceeded to observe the effect of placing a ligature on the femoral vein. The flow decreased (after ligature of the vein) but took on a pulsatile movement. At first the amplitude of the vibrations was about five globules and later it was only two. These vibrations were always exactly synchronous with the rhythm of the heart and would cease as soon as a ligature was placed about the artery. When the ligature was removed from the artery, the rhythmic activity of the globules would promptly begin. Therefore, concluded Poiseuille, the oscillations of the globules were due to the action of the heart. Haller and Spallanzani,¹⁸ two learned physiologists of the eighteenth century, had believed that the heart controlled the capillary circulation. Poiseuille's carefully performed experiments confirmed their belief and also proved beyond any question the cardiac control of the circulation of the blood in the capillaries. Poiseuille also considered the elasticity of the walls of the artery and recognized it as an important accessory to the heart in the control of the capillary circulation.

Next, Poiseuille turned his attention to a study of the character of the flow of blood in the capillaries with the hope that he would be able to explain the variable movements of the globules. Many previous observers had noticed a transparent layer between the wall of the capillary and the moving globules. Some said this layer was an integral part of the wall. Poiseuille proved that this layer was a part of the fluid in the lumen of the capillary by causing it to disappear after the application of ligatures

on the venous side. The transparent layer would disappear on stoppage of the flow of blood. Girard had already demonstrated the existence of an immobile layer of liquid next to the wall of a tube when he was studying the flow of liquids in larger pipes. Poiseuille recognized the same phenomenon in these capillaries and found the immobile layer to be considerably thinner than that calculated by the physicist. Poiseuille found the thickness of the transparent layer to be from an eighth to about a tenth of the diameter of the vessel. This plasma or transparent layer became narrower as the speed of the globules became slower and finally disappeared entirely when the flow was zero. When the frog had been fasted, Poiseuille made the interesting observation that the blood contained a much larger number of small circular globules.

Poiseuille observed that the velocity of the globule depended on its distance from the center or the axis of the blood vessel—that its velocity parallel to the axis was greatest at the center and that it gradually decreased, becoming zero at the wall. This observation was not original with Poiseuille. Malpighi, Haller and Spallanzani had made it previously. However, Poiseuille interpreted these different velocities as indexes of the character of the flow of the blood. He considered the fluid to be divided into several concentric cylindrical layers, the central layer being a thin filament and the outermost layer (a cylinder concentric with this thin filament) the immobile one next to the wall. This type of flow is well known today and various aspects of it are characterized by the terms *viscous*, *steady*, *streamline* and *laminar*.

Knowing that this was the nature of the flow of blood in the capillaries, Poiseuille could easily explain the various movements of the globules. The globules slide along the vessel with the speed of that layer of fluid in which they happen to be located. If a globule is located in the axial layer, it moves with maximal speed. If it is pushed by its neighbors into the marginal layer, it either moves extremely slowly or comes to rest. If part of its volume is located in the immobile layer and part in the next layer, which is moving, it will undergo a motion of rotation. If it finds itself in intermediate layers of different speeds, the globule will undergo both rotation

¹⁸ Lazar Spallanzani, *Experiments upon the circulation of the blood throughout the vascular system; on languid circulation; on the motion of the blood, independent of the action of the heart; and on the pulsations of the arteries*. With notes, and a sketch of the literary life of the author, by J. Toudres. Tr. into English, and illustrated with additional notes, by R. Hall. (J. Ridgway, London, 1801).

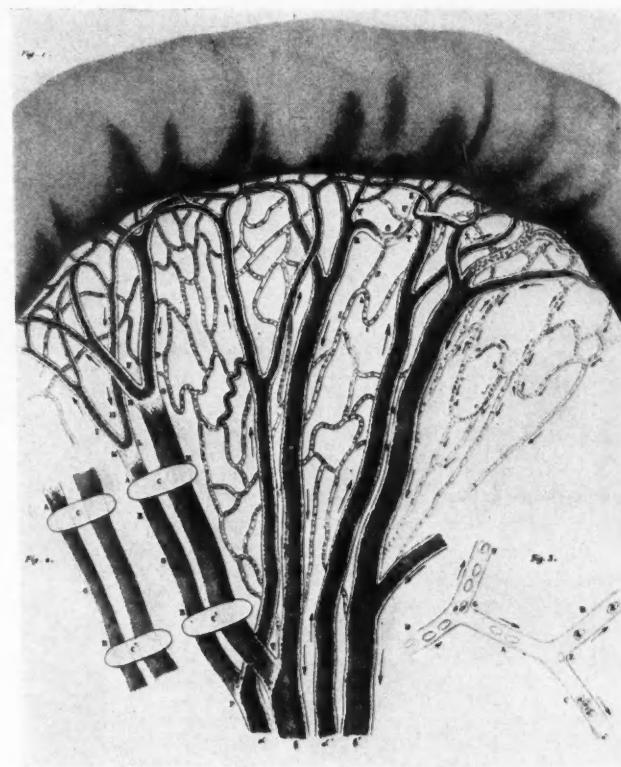


FIG. 1. A copy of Plate I from reference 2(b). Note the absence of the plasma layer in Fig. 2 of this plate which is due to stoppage of flow by C and C'.

and translation. This last idea explains the whirling motion noted by earlier observers. It also explains how the distance between any two globules may sometimes increase and sometimes decrease. Under these conditions it is possible for several globules to come together, forming what is called an *agglomeration*. Such agglomerations could cause a blockage of flow which in turn would cause a reversal of flow (Figs. 1 and 2).

Poiseuille drew the following conclusions from his experiments: (1) the flow of blood in the capillaries is controlled by the heart; (2) the variable movement of the globules is not caused by some spontaneous mechanism within the globule itself but is due to the character of the flow of the liquid (the plasma).

Poiseuille's curiosity in regard to the movement of the blood in the capillaries was not yet satisfied. He wanted to know what effect various temperatures and pressures would have.

Temperatures produced by placing the area including the capillaries in ice water caused either a complete cessation or a marked decrease of flow. If the water was warmed to about 40°C, the flow of blood attained a speed equal to that in the arteries. It is interesting to note that Poiseuille inferred a mechanism for the causation of disease from these observations, that is, that the cold season produces rheumatism.

Spallanzani had an ingenious apparatus for studying the effect of high pressures on certain physiologic processes but he did not disclose its nature; therefore, Poiseuille had to devise one of his own. Poiseuille's publication^{2(b)} gave a complete description of the apparatus as well as an illustration. He subjected his animals to pressures ranging from 2 to 8 atmos as well as to subatmospheric pressures. He knew that newly born mice and rats could survive for a certain length of time without breathing, and this made it possible for him to observe the

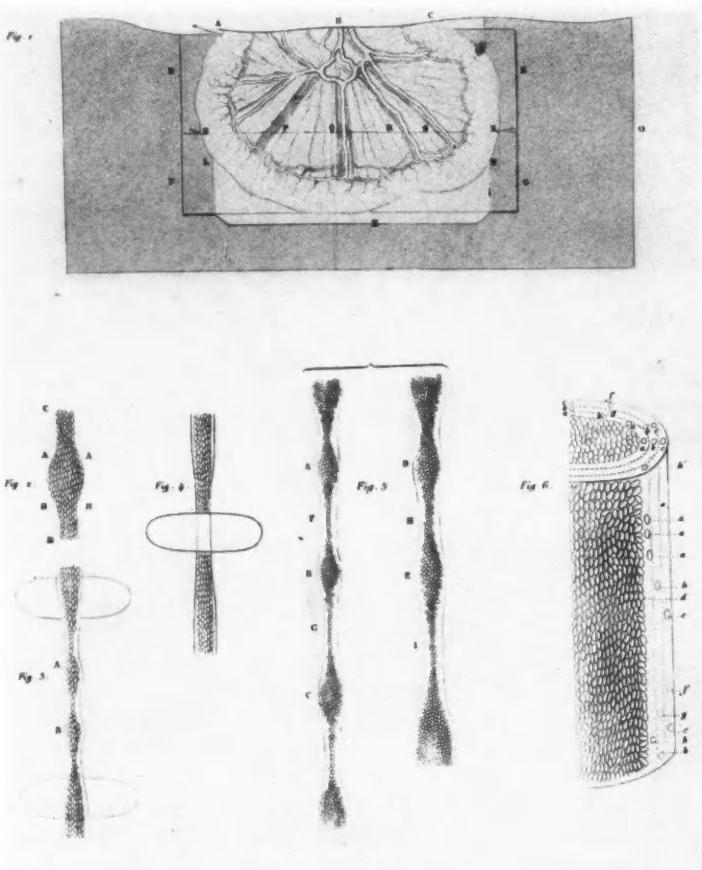


FIG. 2. A copy of Plate V from reference 2(b). Note the globules in the plasma layers in Fig. 6 of this plate.

effect of a vacuum on the circulation. Poiseuille found that the circulation continued with the same rhythm when the animal was subjected to all these various pressures. The thickness of the transparent layer of plasma remained unaltered. These experiments show the incorrectness of the opinion of those physiologists who thought that circulation was impossible without atmospheric pressure.

Previous to Poiseuille's study of the flow of blood in capillaries he had made an extensive investigation of the blood pressure of dogs and horses. It was he who introduced the mercury manometer when measuring the pressure in arteries. He observed the oscillations of the column of mercury with each heart beat. He also observed the influence of the respiratory

movements on the blood pressure. His observations extended to the measurement of venous pressures by means of a water manometer and the variations in venous pressure with respiration. He proved to his own satisfaction that respiratory movements aided the venous return.

With such an extensive study of the circulation of various animals, it is not surprising that Poiseuille became interested in hydrodynamics. In one of his publications he suggested that hydraulicians should use the circulation of animals as a means of learning about the movement of a liquid in tubes of small diameter and thus obtain data which could not be detected elsewhere. Apparently this idea grew on him until he decided to study the flow of liquids in very small capillary tubes himself in order to

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observe the effect of the various controlling factors separately. This was the work that made him immortal.¹⁹

ADDENDA

(1) Poiseuille's law may be found as the *Hagen-Poiseuille* or the *Poiseuille-Hagen* law in some textbooks or other references. Hagen, an engineer, developed the law and published it. Hagen's experiments were few whereas Poiseuille's experiments were numerous, and the fundamental experiments preliminary to those which established his law were published previous to the paper by Hagen. Bingham, Hatschek²⁰ and other critics have expressed the opinion that the law should have the one name of Poiseuille. The reason for the one name can be stated best by quoting Hatschek²¹: "Hagen's work, although practically a complete anticipation, has been overshadowed by that of Poiseuille, a short abstract of which appeared in the *Comptes Rendus* of 1842, his first paper being printed *in extenso* in 1846. No doubt this is largely due to the extra-

ordinary completeness and elegance of the investigation, which still deserves careful study, and the fortunate accident that Poiseuille approached the problem as a physician interested in the circulation of blood in capillary vessels, and not as a hydraulic engineer; he accordingly used [glass] capillaries of very much smaller bore than any of his predecessors and had to deal with purely laminar flow."

Poiseuille was aware of Hagen's work. We find the following footnote in his paper which appeared in *Annalen der Physik und Chemie*^{22(d)}: "Es ist dasselbe Mittel, dessen sich G. Hagen bei seinen Versuchen über die Bewegung des Wassers in zwar engen, aber nicht capillaren Röhren bediente."

(2) A law very similar to that of Poiseuille was developed experimentally for the purpose of calculating the flow of water through sand. The law is called *Darcy's law* after the man who developed it.²³ Some hydraulic engineers think that this law should be called Poiseuille's law because Darcy in his own paper recognized the fact that his law and that of Poiseuille were the same. Darcy thought it remarkable that he and Poiseuille arrived at the same law experimentally under such completely different circumstances.

¹⁹ Biographies of Poiseuille: (a) Ann. de phys. **15-16**, 411-417 (1931); (b) P. L. B. Caffe, J. de conn. méd. prat. **37**, **38**, 62-64, 1070-1071 (1870-1871); (c) M. Brillouin, J. Rheology **1**, 345-348 (1930).

²⁰ Emil Hatschek, *The viscosity of liquids* (Van Nostrand, 1928).

²¹ Reference 20, p. 10.

²² H. Darcy: (a) Compt. rend. Acad. d. sc. **38**, 407 (1854); (b) "Recherches expérimentales relatives au mouvement de l'eau dans les tuyaux," Paris, Mém. Savans Étrang. **15**, 141-403 (1858).

The Contribution of Physics to the College Curriculum

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IN an earlier paper¹ the writer has treated some of the general problems created for the teacher of any of the sciences by recent rapid changes, both in educational theory and in the new demands made by society on education at the college level. These new demands are becoming so insistent that curricular offerings which do not give them the consideration to which they are entitled are in danger of losing much of their opportunity to contribute to general education. The situation in which science instruction in the United States finds itself is, as Professor Taylor has recently pointed out very clearly,² a dangerous one, and it will be truly "the wisdom of self-preservation" to spend some thought on these

problems before the trend away from the sciences develops into a mass movement which would strike the sciences altogether out of the general education curriculum. The present paper applies to physics the general considerations earlier developed for the sciences as a whole.¹

THE TREND AWAY FROM PHYSICS

The attempt to remove the sciences from the curriculum or to deny them their full share in it starts from different, one can say opposite, considerations, according as one considers universities or secondary schools. A university is usually a loose union of faculties and departments with specialist interests, each of them trying to be more or less self-contained. The general physics course is preponderantly a service subject for the professional schools except to the extent that it is an introduction to physics itself as a profession. Medicine and the various branches of

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¹ Not yet published.

² L. W. Taylor, "Science in general education at the college level," Am. J. Phys. **8**, 41 (1940).

engineering, while being fundamentally impatient with physics as ordinarily taught in the colleges, nevertheless give it a sufficient degree of lip service to result in its continuance substantially as the colleges desire to present it; that is, as the first step in the preparation of physicists. The secondary schools, on the other hand, have been expected by the universities to lay the mathematical and elementary scientific foundation for university studies but are now sadly neglecting this task under the beguilement of the "social studies" or outright political propaganda. This is at least partly because educational policies in the universities are governed by learned specialists, whilst in the secondary schools they are directed by educationists and, in the European continental states, by politically minded school ministries. The same difference is being accentuated in the United States by the mushroom growth of the junior colleges.

Notwithstanding the conservatism of teachers of physics, great changes will have to be made in the way that subject is presented if it is to meet the responsibilities placed upon it in modern liberal education. Buttressed by the science requirement, teachers are resolutely closing their eyes to the growing wave of aversion to the sciences in general and to physics especially. We shall do well to cease deceiving ourselves and, as the first step toward avoiding the ultimate elimination of physics from all except the professional schools, ask ourselves honestly why physics is unpopular; why students of medicine, biology, chemistry and even engineering do not like physics; why, above all, liberal arts students avoid it almost as they would a plague. It is merely cultivating our ego to assume the attitude of "pearls before swine." Let us honestly search for reasons why physics would not last a year in the liberal arts curriculum except for the compulsion of the science requirement.

THE PLACE OF LABORATORY AND DEMONSTRATION

There can be no doubt of the real place that physics occupied in the educational scheme at the time it was introduced. Otherwise it would never have been included. Until about 60 years ago physics was mainly mechanics, and this topic was, in the conception of the time, the principal

part of the study of physics. The subject was not very extended; there was time for details and for deeper understanding of the main principles, which at that time constituted the picture of the physical world. Except to a limited degree there were no technical applications of any importance, and electricity was largely a matter of curiosity. Ernst Mach proposed the serious study of some chapters of physics as the means of learning the physical method, since the traits of reasoning in all parts of physics were similar. But a change came with the modern development of physics, with the growing sphere of electrical phenomena, with atomic and subatomic theory, with the identification of the various kinds of radiations and with technology. More details came into the course, but there was no more time to go into them with the same exactness as before, and superficiality was a necessary consequence.

On the other hand there was the tendency to supplement the textbook by a superficial variety of demonstration material. The technical possibilities were enlarged upon, big lecture theaters were built for many hundreds of students, allowing the demonstration of phenomena to great audiences, and really in many cases the course degenerated into mere entertainment.³ This change in the philosophy of teaching physics is strikingly characterized in a remark of Röntgen about the lectures of his successor in the chair of physics at Munich University. He wrote in a letter:⁴

I have heard that Wien intends to give his lectures in a thoroughly elementary way, and that in his first lecture he promised his students not to use any mathematics, but to show them great experiments. . . . In this procedure I can but see a lowering of the standard of the teaching of physics which will be to the disadvantage of the students and of science.

The name "experimental physics" which is given to the first physics course in German universities on account of the experimental and inductive procedure, led more and more to the conception that it involved the obligation to proceed from experiment to experiment largely in the form of demonstration lectures. The student became a visitor to a show, in which his interest was to

³ This holds especially for lecture courses in German universities after the last war.

⁴ O. Glasser, *William Konrad Röntgen* (Charles C. Thomas, 1934), p. 180.

be awakened through curiosity, a term which even today, especially in secondary schools, is a shibboleth of educational theory. The textbook, mirror of the lecture course, became a conglomerate of facts, methods, laws and technical applications. The details were arranged encyclopedically, but an intellectual bond was lacking. At the most the lecturer tried to emphasize the importance of physics in the first or the last lecture, like the textbooks, which in vain tried to outline the far-reaching connections of their topic on pages one and two. But, after meeting with a brief period of student favor, the experimental lecture, as an exclusive way of presenting the subject, is now declining in the favor of students and instructors alike. The mathematical way had been sacrificed because students disliked it. The lecture room had been filled with demonstration experiments in the hope of arousing interest, but ultimately to no avail.

The reason is easy to understand. When lecturers started to make experiments, they could expect that the students would be surprised by phenomena which were at the time outside the ordinary experience. But the physical applications in our common life became more and more usual. The electric light and even discharges in gases no longer commanded interest for men living in a modern town with electric illumination and electric signs—at least before the black-out. In 1900 the famous experiments of Ampère, or a small electric motor, were perhaps interesting objects of a physics course. They are interesting no longer, notwithstanding their continuance in the lecturer's bag of tricks. And so one will hardly find an experiment that is not surpassed in showmanship by technical apparatus of our common life, or by the technical representations familiar in illustrated papers and the cinema. Atomic physics provides no new possibilities, as experimenters like to believe, because demonstrating atomic and electronic phenomena by experiments makes visible only some known facts, for example, a crackle in the loudspeaker, a speck on a phosphorescent screen. Such experiments cannot be compared with those in which one observes the phenomenon itself, for example, the deflection of a compass needle, the dispersion of white light, and so on. Demonstration experiments for the sake of curiosity are therefore

becoming steadily less effective as a teaching technic in general physics. Somewhat paradoxically, they are still useful for more advanced students of physics who are able to see further beneath the surface.

So even the over-emphasis put on demonstration experiments in the lecture has not had the desired ultimate effect. The college course has become largely a repetition of courses in secondary schools, only going farther because of the greater facilities in colleges. It may therefore be understandable that in many places a tendency to remove physics from the curriculum for non-physicists has become evident, along with an alternative demand for more specialized courses for particular professional purposes.

THE CULTURAL APPROACH IN PHYSICS TEACHING

It is in the United States, in the colleges and universities, that a general suspicion seems first to be dawning that something is wrong with the way the sciences are being taught. The continental European educational system is too rigid to allow immediate changes, and only in isolated instances does this awareness seem to have occurred. In Great Britain tradition prevails. But in America, with a private educational system at the higher educational level besides the state universities, public opinion finds more ways to bring its influence to bear. One of the manifestations of this is the creation, by the American Association for the Advancement of Science, of a committee to inquire about science courses on the college level.² In more than a thousand cases studied, the preliminary report of the committee showed that modifications of old courses have taken place to a considerable extent. Though survey courses have been considerably publicized, they seem not to be meeting with very general favor among teachers of the conventional subject-matter fields. Such courses are under the suspicion of "superficiality." Yet there does not appear in the report of this committee any other general trend among the various modifications that are being effected in the traditional science courses. This is a pity, since the literature in this field is rather limited. One hopes that no significant undercurrent of reform is escaping notice.

It is chiefly in physics that the report of the committee finds any measure of unity in the modifications that have been undertaken. This is a tendency to play up the importance of physics as a factor in shaping up the social order. This does not imply an abandonment of the traditional pattern of instruction in subject matter, but rather the injection of the new point of view into it and the emphasis of that point wherever it can be blended with the subject matter. The emphasis seems to take either or both of two forms: in the domain of technic, as "service" and in the sphere of intellectual development, as "progress." The first conception refers to the modern development of physical technology, the second to the presentation of the history of physics. The former is represented by applied physics, the latter by pure physics.

There can be no question that physicists have been associated with technicians in the development of modern technical achievements. Physics has helped to change and to shape our environment. We call to mind Oersted's and Ampère's electromagnetism, Faraday's and Henry's induction, Maxwell's and Hertz's electromagnetic waves, successes of pure research in physics. There are, besides such men as these, the numerous investigators who have developed laboratory experimentation to practical applicability, stimulated by men like Siemens, Marconi and Edison. Nobody can or will deny that. Even the "literati" are eager to utilize physical apparatus, not only to make life more comfortable, but also in their professions—to photograph and film themselves and their works, to fix and reproduce language and music, to broadcast their utterances.

The "progress" conception of the physicists, the second component of the flattering ideas about the importance of their work, is derived from history, and says that the intellectual progress which science creates is embodied in successive new stages of human culture by a continuous process. Though there is a sense in which this assertion can be justified, those who are accustomed to bow down indiscriminately to the great god Science are likely not to set proper limitations to their estimates of its contribution to intellectual progress. The classics over-estimated their place in somewhat this way.

Their pride went before a fall which has notably impoverished the modern program of general education. Men of science will do well to guide their subject into safer channels. Indeed, the whole question of the degree to which the discipline of one intellectual field can be transferred to another is a tricky one. The writer has tried in his earlier paper¹ to show some of the limitations of the prevalent "transfer theory" of education.

The basis of the scientists' belief in progress comes mainly from Comte's historical conceptions, which say that the development of mankind follows the "Law of the three states." Human culture, says Comte, advances from a theological world conception through a metaphysical conception to a so-called positivistic one. Progress in the sense of Comte means reaching the positivistic stage, and astronomy and physics were considered by him as the two sciences that had reached this stage, while the biological sciences and sociology were and are still struggling against metaphysical conceptions. Beginning with Galileo, says the common historical presentation of physics, this subject outgrew theological and metaphysical ideas. Experimental facts alone constitute the basis of physics, so the story goes, and the only task of this science is the synthesis of quantitative physical notions into "laws" or, more exactly, into functional formulations.

Insofar as the modern adaptation of Comte's doctrine leads one to believe that only with the change of physics into the positivistic state does human thinking become liberated from tradition and superstition, one is the victim of a rather serious distortion of the actual course of history. The erroneous consequences of Comte's thesis may be shown in a quotation from his *Sociology*, which was written about 100 years ago and deals with the sociological importance of the sciences and of their technic:

The development of the sciences, of industry and of the arts is indeed the main cause of the decay of the theological and military spirit. The preponderance of the scientific spirit guards us against a revival of the theological spirit; likewise the industrial revolution constitutes the greatest security against a return of the military and feudal spirit . . . neither government nor even school-masterly opinion was reactionary enough to undertake, or even to plan the

systematic displacement of the sciences, the fine arts, and of industry; rather all authorities regarded it their honor to promote their progress. This is the first inconsistency of reactionary politics, which through the development of their activities destroys the plans for a restoration.

It requires only a slight knowledge of history to show that Ernst Mach extended this doctrine to the point of attributing to physics a measure of responsibility for moral and social development in a passage of his famous book, *The science of mechanics*. Reviewing Guericke's work, Mach⁵ says,

Guericke's book [*Experimenta nova, ut vocantur Magdeburgica*] makes us realize the narrow views men took in his time. . . . We perceive with astonishment how short a space of time separates us from the era of scientific barbarism, and can no longer marvel that the barbarism of the social order still oppresses us.

Today, as well, witness can be found of this opinion about the progression from physics to enlightenment. Thus, in an attitude very similar to that of Mach in the eighties,⁶

The decline of the belief in witchcraft owed something to the writings of Ady and Webster. It owed much more to the scientific temper of an age in which "spirits" had been driven from the retort, and the four elements were beginning to be understood in order to be harnessed in the service of man.

The historical reality is rather different: superstition disappeared not through the sciences, and not even through science-mindedness—because science was not, and perhaps never can be common property—but through the activity of enlightened men, like the encyclopedists in France or Thomasius in Germany, whose rationalism and humanism originated from other sources. There are many superstititious beliefs today in spite of electric light and radio. In a recent work Dewey⁷ says very rightly:

It is no longer possible to hold the simple faith of the Enlightenment that assured advance of science will produce free institutions by dispelling ignorance and superstition—the sources of human servitude and the pillars of oppressive government.

It requires, indeed, only a slight knowledge of history to show that the idea that a humanistic

rationalism or a rationalistic humanism was promoted by the natural sciences beginning with the seventeenth century puts the facts upside down. But this is not the worst of it. To entertain such an impression is not merely a delusion. Recent occurrences indicate quite unmistakably that, in exaggerating the cultural importance of physics, one may possibly be venturing upon ground that is actually dangerous. Among the sciences physics is doubtless pre-eminently qualified to illustrate a fundamental line of intellectual enterprise; however, the physics teacher has to navigate between the Scylla of the "service" conception and the Charybdis of the "progress" conception. His task is not a glorification of physics but a showing of it as a branch of the cultural life, and for this task he has duly to prepare himself. Professor Taylor,⁸ in advocating a cultural physics course in the American liberal arts college along historical lines, rightly pointed out that "It will not be [for the teacher] an easy undertaking, and the hardest part will be the education of physics teachers themselves for this task. It will have to be largely a process of self-education. . . ." This holds even more for a course which is intended to prevent the belief that science works automatically for progress. Moreover, we should be aware that both the "service" and the "progress" conceptions have proved themselves very applicable in dictatorial states, where physics appears as a sub-topic of a science of warfare, in which a distorted history of science has to feed national pride while true scientific activity goes into eclipse.

THE BROAD EDUCATIONAL VALUES OF THE PHYSICS COURSE

In stressing the standpoint of pure science one should show the students, who mostly are youthful sceptics, that there is something like an unselfish scientific interest, perhaps another form of curiosity, but, anyhow, a search for knowledge. Whether the scientist motivates his work with a longing for truth, or with his endeavor to recognize God's work in nature, or with his wish to help mankind, should be left to him. Of course, there are scientists whose motivation

⁵ E. Mach, *The science of mechanics*, tr. by T. J. McCormack (Open Court, 1907), p. 118.

⁶ J. Cohen and R. M. W. Travers, *Education for democracy* (Macmillan, 1939), p. 248.

⁷ J. Dewey, *Freedom and culture* (Putnam, 1939), p. 131.

⁸ L. W. Taylor, "Physics in the liberal arts college," Am. J. Phys. (Am. Phys. T.) 6, 315 (1938).

is very much simpler—namely, material recognition and reward—but this does not exclude deeper motivation in other cases. It is interesting that the standpoint of the “applied” physicist who adheres to the “service” conception seems to us more acceptable because we are hardly any longer accustomed to believe in any disinterested activity, either in the personal sphere or in the sphere of scientific work as a whole.⁹ We are today so eager to utilize all possibilities of our material civilization that it is unbelievable to some people that any man's reasoning would be directed to pure cognition as long as material needs are not completely satisfied. In the history of thought, for example, it is almost a truism that the roots of scientific reflections of men in primitive stages of culture have as their sole initial source the satisfaction of material necessities. Nevertheless there is abundant justification today for presenting the sciences *inter alia* as the work of men who are primarily interested in a detached search for knowledge.

The same holds for the importance of scientific international collaboration. Scientific education has, as yet, hardly contributed in very large measure to the spiritual unity of nations, although in a physics course all elements which could be helpful for an approach of this kind should be pointed out. The classical languages once formed a super-national element in education, whose removal from the syllabus we certainly feel in the crisis of Western civilization which we are witnessing, and the collapse of culture in so many countries gives evidence of the work of the man with a so-called “realistic” education. This does not mean that one has to go back to instruction in the ancient languages. Science can introduce in education a super-national element if we show the interrelation of scientific work in the European-American civilization, and it deserves from this fact a place in the curriculum of the most elementary education.¹⁰

Apart from these general considerations which are applicable to other curricular subjects than physics, one more consideration is especially

⁹ See, for example, J. D. Bernal, *The social function of science* (Routledge, 1939), p. 252; M. Polanyi, “Rights and duties of science,” *Manchester Sch.* 10, 175 (1939).

¹⁰ David Dietz, in fact, anticipates the substitution of physics for the ancient languages as the “cultural bond” between educated people; see Dietz, “Cultural values of physics,” *J. App. Phys.* 10, 86 (1939).

important for students whose further education is to be more specialized on the side of the natural sciences, namely, the deeper significance of science and scientific thought. A large number of students in universities will not become men of pure science: the engineers, the physicians, the chemists, and so on, will walk in ways of practical life. But they are the people who by their numbers are decisive in society, more decisive perhaps in regard to practical consequences than the very few original thinkers; they are influential as arbiters who through their judgment guide the development of the genuine thought of the few. It is therefore very important indeed to show what the methods and results of science are, and to destroy any exaggerated idea about science, to prevent science becoming, as it rather is already today, a new sort of magic, a new kind of superstition. The infallibility of science is a conclusion from the opinion that educated people get from their schools or their reading. Francis Bacon very rightly said that “Men mark when they hit, and never mark when they miss;” this applies to scientists especially, who very easily succumb to the temptation to show only the successes of the scientific method, and to veil the trial and error character which governs a great part of scientific and industrial research. The idea that “scientific” thinking and reasoning is so very much superior to that of common life is hardly to be supported. The results of scientific work do not spring ready-made from the heads of logical thinking scientists, and a look into the scientific workshop—or into scientific journals—clearly shows the roundabout ways, the serpentine lines, that scientists take in reaching their results. It is one of the problems of utmost importance that the “scientifically educated” people should acquire a comprehension of how scientific work is done, and that the whole people participate in this realization as far as possible. This is the only way to prevent pseudoscience from becoming dominant, as it really has become in the dictatorships of all colors, where, under the cloak of “scientific,” any idea and ideology is made palatable.

One of the best ways of guarding against the more common misapprehensions about the nature of the sciences lies in an insight into how they grew. This is a fascinating story in its own right

and can be made doubly so when tied in with the study of physics itself. One can outline the beginning of the development of specific physical considerations in nature, starting with the Greeks; the renaissance of scientific reasoning on the basis of the new conception of the intellectual independent power of man in the Middle Ages, the struggle with magic and mysticism, and the eventual victory of a natural science which banished arbitrariness from its considerations and substituted the concept of causality. There are many interesting points of a general character worthy of consideration: the sciences in a prehistoric time; the connection between myth and science; the scientific age of Greece which destroyed the old religion; the schools of higher learning; and the renaissance of science, which was connected with the stripping off of theological bonds. This liberation prepared the great period of science, which may last as long as rationalism can resist the recurrent attacks of mysticism such as are occurring in Germany today. In every century we face the same intellectual problems, and it is right to show students, to whom the appeal of mysticism is stronger than most men of science realize, that both ways of looking at natural phenomena are appropriate, each in its own domain.

It is important to stress the point, sometimes overlooked or not clearly expressed, that the statements of physics have a very wide claim in inorganic and organic nature alike. This does not mean that everything can be explained by physics or that nonphysical considerations are useless. Physics is not confined to a narrow field; it is not only a system of theories and experimental technics by which certain practical activities are possible; but it is a method, applicable to other sciences.

On the other hand it is important also to make it clear that the great political, sociological, economic and religious problems cannot be solved by "science": whether a state system should be democratic or dictatorial; whether discrimination among races is justified; whether a society has to be capitalistic or socialistic. Though men of science may and should take part in public life, no one should expect that their scientific education gives them any particular advantage over other men of similar abilities.

except in areas directly involving their respective fields of competence. The "operationalism" of physics cannot be transferred as easily to problems of life as the representatives of the "Unity-of-Science movement" expect:

If this impartial and fact-minded procedure were adopted also for social organizing and planning, for the revision of individual, national, and international attitudes, considerable progress would be facilitated in the direction of greater security, stability and happiness of mankind.¹¹

These are only pious wishes, and it is very timely indeed when F. A. v. Hayek¹² points out the results of a similar "scienticism" in France after the Great Revolution.¹³

The number of objections that are often urged against broadening the horizons of physics instruction in the ways here suggested are astonishing. There seems at present not the slightest chance to bring about a change in universities in regard to the educational movement, or to change anything in the physics course. Many hindrances will be enumerated: old established regulations; lack of means and of skilled and willing staff; requirements of examinations, and so on. Even scientists who sympathize with a change are rather waiting for a change from without, through political events perhaps, than attempting to do what could be done in their own domain. The great experimenters in science are not prepared to experiment in the sphere of their own educational opportunities, and it is today as it was a hundred and more years ago, when Matthew Arnold¹⁴ said

. . . while the leading humanists . . . have been also school-masters, and have brought their mind and energy to bear upon the school-teaching of their own studies, the leaders in the natural sciences, the Davys and Faradays, have not.

Whether the educational possibilities of the natural sciences are to be developed in a way that they are emphatically not being developed at present, depends on the attitude of teachers and of the institutions preparing them. The

¹¹ H. Feigl, "The significance of physics in man's philosophy," *Am. J. Phys. (Am. Phys. T.)* 7, 327 (1939).

¹² F. A. v. Hayek, "The counter revolution of science," *Economica* 8, 9 (1941).

¹³ On the scienticism of Saint-Simon compare also J. T. Merz, *History of European thought in the nineteenth century* (Blackwood, 1914), vol. iv.

¹⁴ Matthew Arnold, *Schools and universities on the continent* (Macmillan, 1868), p. 261.

broader aspects of our educational responsibilities are far from being fully realized and are continually meeting with the inertia and even the prejudice of men of science themselves. What needs to be done if the sciences are to attain their proper place in the educational scheme goes far deeper than the inauguration of "cultural" or "survey" courses. The broader elements must permeate the whole structure of routine instruction at every level. This can only occur in consequence of a vision which all too few teachers

have caught, up to the present, but which lies within the reach of all. *Without* that vision the sciences will continue their preoccupation with the mere preparation of narrow specialists and will also continue the present trend of gradually disappearing from the offerings of the liberal arts curriculum. *With* such a vision the sciences may look forward to making a major contribution in the construction of a rational and ethical community of Man through preparing a way for it in the very heart of higher learning.

Projector for Stereoscopic Pictures

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NOW that polarizing plastic sheets are available at small cost, the projection and viewing of stereoscopic pairs in black and white or color is technically a simple matter. The Bausch and Lomb Optical Company has long had on the market a projector suitable for this purpose, provided Polaroid was substituted for the red and green glasses, but unfortunately the machine had to be modified were it to take the two pictures mounted between glass covers as a single unit; moreover, it was made for the $3\frac{1}{4} \times 4$ in. "standard lantern slide, and the cost of colored positives for this is high.

When the Kodaslide A 2×2 -in. projector (200-w T-10 bulb) was replaced last summer by the smaller and current models No. 1 and 2, it was possible to secure a pair of the earlier models at \$20.00 each. This projector is a nearly ideal one for conversion to stereoscopic use, since the large lamp house and base permit the insertion of 500-w T-10 bulbs with no modification in the new unit. Unfortunately the condenser barrel is $2\frac{1}{16}$ in. in diameter, which necessitates stereoscopic slides $2 \times 4\frac{3}{4}$ in. The size $2 \times 4\frac{1}{2}$ in. would be slightly better for individual study of the slide in a hand stereoscope, since the centers of the two positives would then be but $2\frac{1}{2}$ in. apart rather than $2\frac{3}{4}$ in. as in the size adopted.¹

¹ The diameter of the largest lens in the condensing system of the Spencer MK2 projector is $2\frac{3}{4}$ in., and since this projector comes with a T-10 bulb, it would seem to be suitable for conversion to a stereoscopic projector taking a $2 \times 4\frac{1}{2}$ in. slide by suitably modifying the directions given herewith.

Figure 1 gives a general idea of the instrument and serves to bring out many of the details of construction. Adjoining portions of the sides and bases of the two projectors were trimmed away so that the condenser barrels were only $\frac{1}{16}$ in. apart, and then each projector was

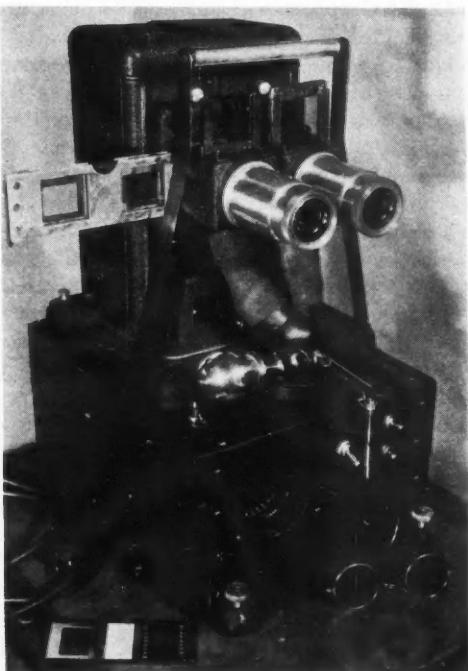


FIG. 1. Stereoscopic projector.

mounted on a metal-plate base; the whole rests on a metal box carrying a twin fan of the turbine blower type. The fan box consists of a pair of $12 \times 10 \times 3$ in. metal chassis for radios, obtainable at any radio supply house. The trouble light fastened to the upper half of the front of the base is the standard typewriter style. The machine, including a carrying case, was built by the writer in spare time during three weeks. The projector weighs about 30 lb (one-third of which is due to the motor blower) and the total material cost was approximately \$70.00.

The mode of construction was as follows. The two projectors were stripped of their "fronts" (the upright portions to which are fastened the cubelike "slide-holder boxes" into which the objective barrels are screwed) and their light housings. The slide-holder boxes (held by four screws through the "front") were removed. As this projector is of the vertical type, in which the slides move down on changing from one to the next, the slide-holder boxes were rotated 90° about the axis of the optical system. Since the lower side of the slide-holder box had a door in it which served to darken the screen while the slides were being changed, the right² slide-holder box was rotated clockwise, the left anticlockwise, so that these two doors (which were fastened in fixed position in the slide-holder boxes) are nearly against each other in the finished projector. To accomplish this rotation it was only necessary to drill four new holes in each of the projector fronts. Care must be used in locating these holes, as the centers of the original holes lie along a circle of diameter 3 in. whose center is offset $\frac{3}{4}$ in. above the optical axis.

A slot was cut near the middle of the top of each slide-holder box; these slots hold the Polaroids, which consist of J-film mounted in Canada balsam between a pair of 2×2 -in. slide glasses. These are held in small metal frames which may be raised or lowered at will along tracks fastened to the inside of the slide-holder boxes. The one on the left-hand projector is shown raised in Fig. 1; it is held in this position by friction between pairs of upright tracks. An elliptical hole was cut in the base of each slide-

² All references to "right" and "left" are as they appear to the operator standing behind the projector and facing the screen; this is a reversal from their apparent position in Fig. 1.

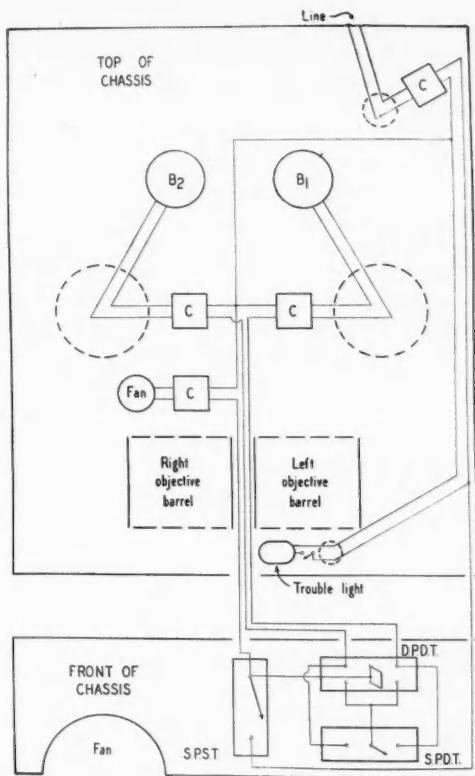


FIG. 2. Wiring diagram.

holder box to take a short piece of metal tubing which is connected by rubber tubes to the Y-shaped junction above the front-blower outlet to permit a forced draft. The air strikes on both sides of the Polaroids and on the front side of the lantern slide.

The adjoining sides of the fronts and bases were next cut off with a hack saw so that, when reassembled, the two slide-holder boxes would be separated by about $\frac{3}{16}$ in. Since this necessitated removing half of the corner supports from the bases of the projectors, these bases were mounted on $\frac{3}{2}$ -in. thick brass plates having holes of diameter 2 in. through which wires might lead from the chassis box to the lamp bulbs. These holes were large to permit adjustment of the reflector supports through the top of the chassis box; of course, matching holes were cut out of the top of the chassis box (large dashed-line circles of Fig. 2). Because the

trimming of the side from each front resulted in the loss of one of the screw threads used to hold the front to the base, a small piece of angle iron was mounted near the cut edge to replace this.

The two radio chassis boxes were fastened together by bolts through vertical metal strips at the four corners. But first the motor-driven twin blower (Delco No. 2373) was mounted on rubber gaskets fastened to crossbars in the lower chassis so that the pipes from the two blowers pointed vertically upward. The front one emerges through a hole at the Y-shaped junction below the two rubber tubes (Fig. 1), and the rear one comes up between the two 500-w lamp bulbs, so that the air cools these as well as the condensing systems. The upper chassis box was wired as shown in the plan and front view of Fig. 2, where B_1 and B_2 represent the two lamp bulbs, and the four squares marked C indicate connecting plugs and sockets, such as are used to join the electric circuit of a trailer with that of an automobile. These are desirable since they permit the apparatus to be taken apart readily. The four dashed-line circles show where wires go through the top of the fan box; since the fan is fastened to the lower chassis box, the wires to it also lead through a connector.

The three toggle switches used appear in Fig. 1; their types are indicated in the lower part of Fig. 2. The single-throw one is of 1000-w, 10-amp capacity and serves as a main on-off switch (not controlling the trouble light, however). The two double-throw switches are so arranged that when they are both to the left, only the left-hand light burns; when both to the right, only the right-hand bulb is lit; and when crossed either way, both lamps are on. This permits use of the instrument to show stereoscopic pairs, or to serve as a dissolving view, single projector.

The metal plate on the base of the left-hand projector is firmly bolted to the top of the fan box; that under the right-hand projector is fastened to this box by a single loose "pivot" bolt directly below where the fronts of the light-housings of the two projectors meet. The right-hand projector can thus be pivoted about a vertical axis through a small angle (1° is amply large) to allow for different screen distances or poorly mounted positives. A wing nut on a

bolt through a circular slot in the metal base plate, shown in Fig. 1 near the front and at the top of the fan box just below the end of the shade of the trouble light, serves to fasten this base plate in any given position.

The lamp housings were next trimmed and their edges bent so as to give a single large housing for the twin bulbs. The housings were rigidly fastened together by bolts at the front, but were loosely held by bolts through slotted openings at the rear; this arrangement allows for a small rotation of the right-hand projector. The tops of the housings were similarly cut away, bent and then fitted together into single units. A slide carrier (see Fig. 1) was made to take two double slides, each $2 \times 4\frac{3}{4}$ in. This runs in a track supported by both slide-holder boxes but fastened only to the one of the left-hand projector; this type of mounting permits a small rotation of the right-hand projector. Moreover, an ordinary single-projector horizontal slide holder was cut in two so that when the slide carrier shown in Fig. 1 was removed, the proper half could be inserted in its place from the right or left; in this way the projector serves as an excellent dissolving view device for ordinary single slides; under these conditions the Polaroid sliders in the slide-holder boxes are raised above the light path. Figure 1 also shows a handle of great aid in moving the projector about. Two of the adjusting screws supplied with the original projectors were remounted at the base of the front corners of the fan box (well shown in Fig. 1) to permit centering the images on the screen.

The 500-w bulbs furnish so much heat that even with the double blower used it was necessary to cut the heat-absorbing glasses into three strips. The Corning light shade Aklo No. 396 glass ($\frac{1}{4}$ in. thick) originally supplied with the projector was satisfactory, but if the extra light shade (No. 395) is used, the films will melt if left in very long. It is important to dry the films and varnish them (as is done by Eastman to the Kodachromes before placing them in the ready mounts), else the gelatin is likely to melt.

The projector gives satisfactory illumination in a fairly dark room at a distance of 20 ft. Under these conditions, using a General Electric Company exposure meter without a filter, readings of 6 ft-c were obtained at the center

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of the screen (polarized light, No. 396 Aklo glass heat-absorbing filter, one bulb, no slide). When the original lenses were replaced with Bausch and Lomb $f:3.5$ Series O Cinephor 5-in. lenses, this was increased³ to 10.5 ft-c. This intensity gives very satisfactory screen images from 35-mm Kodachromes at 30 ft in a dark room. These conditions are suitable for an audience of 50 to 75 supplied with cheap cardboard Polaroid "glasses."⁴ Of course, a beaded or white or any other depolarizing screen must not be used. A good opaque screen that has been sprayed with aluminum paint is quite satisfactory.

Because it is desirable that the lecturer be in the back of the room in order that he may properly see what he is talking about, the projector has been modified since the photograph (Fig. 1) was made so that a light arrow mounted on a universal joint is part of the equipment. An old De Vry motion picture projector was converted for this purpose, the 6-v transformer being added to the basal part of the fan box. Unfortunately, since unpolarized light is used, the image of the arrow on the screen may seem to be in front or behind the object which one is attempting to point out. It would be desirable to modify this so that a double arrow (polarized light) of variable apparent parallax could be projected.

The writer has taken many stereoscopic pairs in Kodachrome with an ordinary 35-mm camera. A cross hair graticule has been mounted on the lens shade so that it sticks up in front of the view finder. It is thus possible to take two pictures from different positions pointed at the same spot. Moreover, the horizontal hair is of great help in holding the camera in a horizontal position. The distance between the camera stations may be varied from 1 to 5 percent of the distance to the object, and the line joining the two camera positions should be approximately normal to the lines of sight to the object.⁵

³ According to a current advertisement this intensity could be increased some 30 percent by using the recently developed H-film instead of the old J-film for polarizing purposes. Of course, with the new $f:2.0$ coated-surface lenses the screen intensity would be greatly enhanced.

⁴ Obtainable at a cost of a few cents each from the Society for Visual Education, which has recently marketed a three-dimensional projector. Unfortunately this is for vertical slides which cannot be studied in a simple, small hand stereoscope. See Rev. Sci. Inst. **12**, 41 (1941).

⁵ F. E. Wright, J. Wash. Acad. Sci. **14**, 72 (1924). However, note that when one is taking a general view (and not a picture of a single object), the distance between the two camera stations should not exceed 5 percent of the distance to the nearest object that will show in the foreground, and a 3-percent maximum is a safer figure if easy fusion for the average person is wanted.

The two stereoscopic pairs should be mounted in a single glass slide. Suitable $2 \times 4\frac{1}{2}$ -in. slides cut from so-called microscopic glass can be obtained from large dealers in glass at 3 to 5 cts each, depending on the quantity ordered. Each picture is first mounted in the ordinary mask⁶ and these are then fastened with a strip of Scotch tape to the slide so that one side of each mask lies along a short side of the slide, leaving a blank space $\frac{3}{4}$ in. wide at the center. This may be covered with a strip of paper containing a title. Boxes for the storage of such slides are easily made from ordinary 2×2 -in. slide boxes which have a removable wooden partition down the center.⁷

A suitable hand stereoscope for observation of such slides is simply fabricated from plywood. Lenses of diameter 2 in. and focal length 77 mm are cut in two in the middle and mounted with the thin sides toward the nose and 39 mm apart. These should be 80 mm from the pictures, which should be backed by opal glass in front of a couple of 15-w Mazda D or 10-w S14 clear bulbs. Under these conditions many types of photographs or drawings⁸ may be examined at leisure.

Acknowledgments are made to William Schmidt, technician, for advice and assistance, to Charles M. Riley and NYA for the drafting of Fig. 2, and to Clyde Prusman for aid in devising the circuit of Fig. 2.

⁶ The writer cuts an ordinary folded mask (all of which should be made of white paper coated with aluminum on one side) in two along the fold. He then fastens one edge of a positive in suitable (but reversed) position along the coated side by means of a strip of Scotch tape. This is placed on a glass-topped table lighted from below, and the second part of the mask (aluminum side up) is superposed on it, with all edges coinciding. It is then a simple matter to fit the other positive on this mask so that points in the foreground match one on top of the other—that is, near-points coincide; it is fastened in this position to the mask with a strip of Scotch tape. The finished slide when held in proper position for viewing in the hand stereoscope should have the white side of the masks toward the observer. This matter of framing of stereographs is discussed by J. T. Rule, J. Opt. Soc. Am. **31**, 332 (1941).

⁷ The total material cost of a 35-mm Kodachrome stereoscopic glass slide as described is about 30 cts. When and if vectographs in color are available at no great increase over this price they can successfully compete with this ordinary type of slide for noncommercial work. See E. H. Land, J. Opt. Soc. Am. **30**, 230 (1940). Special boxes to hold fifty $2 \times 4\frac{1}{2}$ -in. slides were made up for the writer at \$1.50 each by the Keene Sample Case Co., 202 W. Lake Street, Chicago.

⁸ See F. C. Sauer, J. Opt. Soc. Am. **27**, 353 (1937); Rowlands and Killian, Tech. Rev. **39**, (5) 1 (1937); A. W. Judge, *Stereoscopic photography* (American Photographic Publishing Co., 1935).

NOTES AND DISCUSSION

War Training in Physics

HOMER L. DODGE¹

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A TWO-DAY session of the National Advisory Committee for Engineering, Science, and Management Defense Training (ESMDT) was held on December 19 and 20, 1941, in Washington. Although many of the decisions reached as a result of this meeting will be known to departments of physics through other channels by the time this note is published, a summary of the situation as it concerns physics may be appropriate.

(1) In July, 1941, the fields of physics, chemistry and production supervision were added to the Engineering Defense Training program which had been in operation during the college year of 1940-41. The place of physics in the normal ESMDT program was clearly stated in an admirable article by Dr. I. H. Solt of the Washington staff.²

(2) At the October 1941 meeting of the Advisory Committee the member for physics was charged with the responsibility of encouraging, with the informal advice and assistance of the officers of the American Institute of Physics and founder societies, the setting up of special centers in the summer of 1942 for full-time training. It was directed that the programs should be confined to departments especially well qualified in personnel and equipment to offer work in a particular advanced field of immediate concern to the war effort and that the number and geographic distribution of centers in any field should be such as to insure reasonably large enrolments.

This was done in as thorough a way as the emergency conditions would permit and, by the middle of December, a dozen such centers were assured. Any department that has not been approached, that regards itself as especially qualified in staff and facilities for such work, and that is reasonably sure of being able to recruit able students, should not hesitate to make application.

(3) The advanced programs at special centers are in addition to the regular offerings expected under the ESMDT program. It is felt that many departments of physics have failed to make the contributions to training for the war effort that they should. Each department should study the needs for physics training in its locality, work out a plan for training that will meet this particular need and submit it to the ESMDT staff for approval.

(4) At the December meeting of the Advisory Committee arrangements were made for conferences on war training in physics at the Princeton and Dallas meetings which it is hoped will have proved helpful to departments in planning their contributions to the ESMDT program.

(5) There is practical certainty that engineering colleges will enter upon an accelerated program affecting the present senior and junior years and strong probability that the same plan will also be followed for the sophomore, freshman and succeeding classes. It is expected that funds will

be available to take care of the additional costs to institutions and to provide loans to students. In institutions where this accelerated program is adopted, departments of physics will be expected to cooperate by offering courses in the *engineering* curriculum to fit the new schedule. It will also be expected that departments of physics will similarly accelerate their own curriculums insofar as possible.

(6) It is expected that the necessity for recruiting able students into engineering, physics, chemistry and production supervision will be officially recognized in the immediate future. College students who are studying in fields not essential to the war effort may well be encouraged to change to the fields mentioned. Secondary school students who have the interest and aptitudes necessary for such work should be urged to prepare for training in these technical fields.

(7) The situation may be summarized in one statement which applies now and will apply until the war is ended: *The demand for physicists well grounded in subject matter related to the war effort and at all levels of attainment cannot possibly be satisfied.* Therefore, every effort must be made to accelerate the preparation of physicists and to recruit to physics training as many students as possible. We should not hesitate to encourage able students from the secondary schools and from other college fields to enter physical science, for there is every reason to believe that their contributions will be needed in the post-war period as well as in the present emergency.

¹ Member for physics, National Advisory Committee for Engineering, Science, and Management Defense Training.

² Solt, Am. J. Phys. 9, 294 (1941).

Determination of the Refractive Index of a Liquid

ALBERT MAY

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G LADDEN¹ has suggested a simple method for determining the refractive index of a liquid, in which a watch glass partly filled with the liquid is mounted as a lens on a vertical optical bench. This method is suitable for use in the elementary optics laboratory but has the disadvantages that the watch glass is constructed of inferior glass of varying thickness and has surfaces that are only approximately spherical.

An arrangement described by Clay² is somewhat superior in that good quality glass and ground surfaces are involved. In this method a plane mirror is placed face up at the bottom of a vertical optical bench, and a biconvex glass lens is laid on the mirror. The liquid to be tested is inserted between the lens and the mirror. An object is mounted above the lens and its position adjusted until object and image coincide first without the liquid and then with the liquid present. Assuming that the lenses are thin the two lens-object distances are required and also the radius of curvature of the bottom lens surface. If the lens-object distance is determined for a second liquid, the radius of curvature need not be known. In this case a comparison of the refractive indexes of the two liquids is obtained.

The purpose of the present note is to describe an arrangement that is similar to these and is both simple and precise.

Mounted on a vertical optical bench are arranged, starting at the top: a lamp bulb, a combined object and screen, and a front surface concave mirror. The object consists preferably of a lantern slide containing a photograph of cross-section paper. This is not bound to another glass plate, but a piece of opaque white paper is pasted over half the slide to act as a screen. The lamp bulb serves as an illuminant for the slide.

Only two principal measurements are required. The first is made by adjusting the slide vertically until a sharp image of it is focused on the adjacent section of paper. The mirror-screen separation is, of course, the radius of curvature r of the mirror.

For the second reading a transparent liquid is poured onto the mirror surface, forming a plano-convex liquid lens. The screen is again adjusted until a sharp image is obtained, and the mirror-screen distance r' read.

It is easily shown that

$$n = (r-t)/(r'-t),$$

where t is the axial depth of the liquid. The only assumptions made are that the rays are paraxial and that a negligible part of the liquid surface is distorted by surface tension.

An accurate measurement of the depth t is not easy to make, and its calculation from the radius of curvature of the mirror and the horizontal diameter of the liquid lens is impractical because of surface tension. If the mirror is filled to the edge with liquid, a cathetometer may be used to measure the distances from the liquid surface to the two positions of the screen. In this way $r-t$ and $r'-t$ are found directly.

For precision, filters should be used with the white light or a monochromatic source should be provided. A separate support may be required for the mirror if agitation of the liquid surface becomes troublesome during the adjustments.

Unless the measurements are to be made with considerable precision a good thin-glass back-silvered mirror will be satisfactory and the lenses may be regarded as thin. In this case $n=r/r'$.

The derivation of either of the formulas may be required of the student.

¹S. C. Gladden, Rev. Sci. Inst., **4**, 231 (1933).

²R. S. Clay, *Treatise on practical light* (Macmillan, London, 1911), p. 143.

Volume by Overflow

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A WELL-KNOWN experiment in the introductory physics laboratory is to determine the volume of some object by immersing it in a vessel of water and weighing the water that overflows through a spout. In our laboratory the results obtained by students have varied so

widely that it seemed worth while to seek the cause. Employing the same apparatus, my results have differed almost as widely as those obtained by the students. The brass block that was immersed had a volume of about 9.4 ml, and the average deviations of five successive determinations was usually about half a gram.

The most obvious factors on which the accuracy of a determination depends are (1) the diameter of the overflow can, (2) the magnitude of the surface tension, and (3) the shape and size of the spout.

With an overflow can 8 cm in diameter the discharge of 0.5 g of water lowers the level of the water surface by only 0.1 mm. It is obvious that the greater the diameter of the can the smaller is the accuracy that can be expected, and also that it is important to have the can on a support that is not subject to vibration.

The effect of surface tension was examined by using the can after thorough cleaning, using it with the inside coated with paraffin, using cans of different materials, using instead of a tubular spout a troughlike one, and using a solution of 25 percent ethyl alcohol instead of tap water. With the alcohol solution the surface tension was so weakened that the final drops came at increasingly longer intervals, making it difficult to know whether the last drop had fallen. In some cases occasional drops fell for more than 4 min. Aside from this intermittent dropping of the alcohol solution no results were obtained that finally seemed to be related in any important way to changes in surface tension.

The spout is important. Overflow cans from three different manufacturers were tried. The one that gave the most consistent results has the smallest spout. This can has a diameter of 7.2 cm and a spout bore of 6.5 mm. With this can seven series of from five to ten measurements gave average deviations that ran from 0.04 to 0.34 g, and averaged 0.19 g.

A can was made with an inside diameter of 6.3 cm and a brass spout that had a bore of 6.1 mm. With this can two series of ten measurements each gave equal average deviations of 0.05 g. The final drops came slower and slower, the interval between the last two running from a little over 30 sec to a full 4 min. It is important that the last drop be caught, for each drop from this can has a mass of about 0.05 g.

The need of these considerable waits for the last drop was avoided by using for the overflow cup a Pyrex beaker of diameter 6 cm, in which the spout is a siphon with a 6-mm bore that passes through the side wall. In different tests the outlet was drawn down to different diameters. The most consistent results were obtained with an orifice of about 1.6 mm and rather thin wall. If the orifice is too small the immersion of the block does not start the flow. With this beaker the drops follow each other rapidly, and there is no question about the last one. After it has fallen the remaining column withdraws slightly in a characteristic manner.

In a few series I have immersed larger objects. These gave average deviations which are practically the same as those obtained with the block usually employed, so that the use of a larger object reduces the percentage error.

Although the beaker with the siphon and narrow orifice is more difficult to clean and more easily broken, it is the most satisfactory of the overflow vessels that I have tried. In two series of five measurements each, and two of ten measurements each, the average deviations were 0.02, 0.05, 0.04 and 0.07 g.

Women Physicists in American Men of Science

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THIS note is prompted by a study made of data pertaining to women physicists as listed in the fifth edition of *American men of science* (1933).

The number of biographical sketches appearing in the fifth edition is something in excess of 22,000. Of this number, 1651 are those of men and women whose field of endeavor is given as physics. Of the 1651 physicists, only 51, or 3.1 percent, are women.

These 51 women were employed as follows: research, 8; industry, 2; teaching, 36; retired, 1; no indication, 4. Seven women—13.7 percent—reported themselves as married. Other data of interest are listed in Tables I and II.

TABLE I. Geographical distribution.

	West of						Not Given
	North	South	Mississippi	Canada	Foreign		
Birthplace	27	5	13	3	2	1	
Location of institution granting degree	66	9	39	7	7	—	
Place of employment	27	7	6	1	3	7	

TABLE II. Degrees held.

B. L.	B. S.	A. B.	M. Sc.	M. A.	Ph. D.	D. Sc.	M. M. E.	Total
1	10	41	10	29	33	3	1	128

In a similar study of the sixth edition of *American men of science* (1938), A. Wupperman¹ found 28,000 scientists listed. Forty women gave physics as their field, one gave optics and one gave physical metallurgy. Of these 42 women, 29 held the doctorate and 11 held only the master's degree; 7 were engaged in research or technical work, 31 were teaching and 4 were in the government service.

The information obtained from these studies appears to substantiate the data and conclusions given in an earlier article.² It is our intention to make a similar study of the seventh edition of *American men of science*.

¹ J. Chem. Ed. **18**, 120 (1941); digest in Am. J. Phys. **9**, 198 (1941).

² Daffin, Am. J. Phys. (Am. Phys. T.) **5**, 82 (1937).

Speed and Distance as Physical Terms

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MOST textbooks¹ distinguish clearly between *speed* and *velocity*. In fact, of 21 books investigated, only 5 fail to make this distinction. However, of the 16 which do make the distinction, only 4 say that the "speed of light" is 3×10^{10} cm/sec. Of the remainder, 3 use speed and velocity indiscriminately, and 9 use the expression "velocity of light" exclusively. The same confusion exists with respect to the velocity or speed of sound, the velocity or speed of efflux of a liquid from an orifice in a tank, and in other connections.

This situation, although unsatisfactory, did not seem very important to the present writer until recently, when there appeared on one of the *Cooperative physics tests*² the following question:

Which one of the following quantities is a vector?
 (1) Angular velocity, (2) Work, (3) Speed, (4) Length,
 (5) Power.

Thus these national tests are emphasizing a distinction in terminology on which physicists themselves do not agree and which is not used consistently even by those who do agree.

Two courses seem open. First, all physicists might agree to define and use *speed* always and only as the magnitude of the velocity vector and to train themselves accordingly in its usage. The objections to this procedure are first, that it would be difficult to persuade physicists and students always to follow this rule in teaching and discussion; second, that it is a rather special procedure for the velocity vector, and the student may wonder why separate names are not given to the magnitudes of acceleration, force, momentum, angular velocity, and other vector quantities as well. The only other vector quantity treated like *velocity* seems to be *displacement*, the magnitude of which is sometimes called *distance*.

The other course is to speak of the *magnitude of the velocity* and the *magnitude of the displacement* in the same manner as one does with all other vector quantities and not to give speed and distance any precise physical meanings.³ They may be mentioned casually as everyday words for these magnitudes and occasionally used where no ambiguity would arise. Under no circumstances should they be tested if this alternative is accepted. This course seems decidedly preferable because of its uniformity for all vector quantities and because of the difficulty of carrying out the first alternative. The writer therefore recommends that *speed* and *distance* be dropped as precise physical terms.

¹ Also Weld, *Glossary of physics* (McGraw-Hill, 1937).

² Mechanics, Form D, item 20.

³ *Speed* would of course retain its meaning in connection with photographic films and lenses.

Nourished by knowledge patiently won, bounded and conditioned by co-ordinate reason, the imagination becomes the prime mover of scientific discovery.—JOHN TYNDALL

Proceedings of the American Association of Physics Teachers

THE PRINCETON MEETING, DECEMBER 29-31, 1941

The eleventh annual meeting of the American Association of Physics Teachers was held at Princeton University, Princeton, New Jersey, on December 29-31, 1941. The presiding officer was Professor A. G. Worthing, President of the Association.

A joint dinner with the American Physical Society was held at the Nassau Tavern on Tuesday Evening, December 30. Other features of the meeting were a seminar on theoretical physics, held in the Institute for Advanced Study; a reception and tea at the Graduate College, at which the members of the two societies were the guests of Princeton University; a buffet luncheon for the ladies at the home of Professor and Mrs. H. D. Smyth; and a luncheon meeting of the Pennsylvania Conference of College Physics Teachers, held at the Nassau Tavern.

The annual business meeting of the Association was held on Tuesday morning, December 30, in Palmer Laboratory; the minutes of this meeting and of the meeting of the Executive Committee of the Association will be published in the April issue.

In a ceremony which followed the business meeting and which is described elsewhere in this issue, Professor R. M. Sutton and President Worthing presented the 1941 Oersted Medal to Professor Henry Crew.

Symposiums, Reports and Invited Papers

During the sessions on Monday, December 29, the following program was heard:

Symposium on college physics textbooks. (a) *Tendencies suggested by the nation-wide physics tests*, C. J. Lapp, *State University of Iowa*; (b) *Limitations of material for a one-year college physics course*, H. D. Smyth, *Princeton University*; (c) *The role of definitions and units*, E. M. Pugh, *Carnegie Institute of Technology*; (d) *A possible future trend in physics textbooks*, L. W. Taylor, *Oberlin College*.

Committee report—The cooperative approach to the problems of science teaching in the secondary schools. K. Lark-Horovitz, *Purdue University*.

Three invited addresses were heard during a joint session with the American Physical Society on Tuesday afternoon, December 30:

Liquid structure. G. W. Stewart, *State University of Iowa*, President of the American Physical Society.

Physics at Princeton, 1890. Henry Norris Russell, *Princeton University*.

War problems of the physics teacher—First Richtmyer Memorial Lecture of the Association. Arthur H. Compton, *University of Chicago*.

Professor W. F. Magie, because of illness, was unable to give his scheduled address on "Joseph Henry."

Contributed Papers, with Abstracts

SESSIONS on Monday afternoon, December 29, and Tuesday morning, December 30, were devoted to the following contributed papers:

1. Report on three years teaching of general physics without textbook or mimeographed notes. Ray L. Edwards, *Miami University, Oxford, Ohio*.—The condemnation, embodied in the remark that teachers who use the lecture method have not yet caught up with the advances made possible by the discovery of printing, is indeed applicable if the lecture process consists of transferring material from the notebook of the instructor to that of the student. Indeed, the instructor of a lecture demonstration course should never bring a note to class. Although there are disadvantages in not using textbooks or printed notes, including somewhat slower progress in covering subject matter, there are also outstanding advantages, such as the student's recognition of the importance of regular attendance and riveted attention, the stimulation to think aggressively rather than to learn by the absorption process and, in the opinion of the author, a more rapid comprehension of fundamental principles and a corresponding increase of interest. Students should of course have ready access to the best textbooks in general physics. At Miami University many of the ablest students in the institution are being attracted into the general physics course, and a large portion of these continue with physics. Yet, for only 5 hrs credit they attend five 1-hr demonstration-lectures and a 2-hr laboratory each week, and also blackboard drill at irregular periods. The method requires considerable extra work on the part of the instructor, such as frequent inspection of notebooks and grading of at least a part of every examination paper to insure his keeping in contact with the mental processes of his students. Obviously the method is not suitable for an inexperienced or indifferent teacher.

2. Course content for general physics for engineers. R. F. Paton, *University of Illinois, Urbana, Ill.*.—The author recently examined the textbooks and syllabuses used in engineering extension courses by the Pennsylvania State College, the object being to determine suitable content for a 14-wk foundational physics course that would serve as preparation for further study in specialized fields. This examination revealed the lack of a suitable physics textbook for such a course and raised the question of the appropriateness of the material in physics textbooks for college classes of sophomore engineers. Yet many administrators consider physics a "cultural" course for engineers, and industrial leaders are pleading with physicists to "come out of their academic cloisters" and help equip students with "useful" knowledge in the present emergency. In view of the topics treated in these engineering extension courses, it is clear that physics textbooks neglect the important field of properties of materials, that engineers have a dangerous blind spot where vibratory phenomena are concerned and that engineers do not distinguish clearly between mass and weight; physics textbooks ordinarily discuss "mass and weight" in a single paragraph, and it is not surprising that the student usually considers

the terms to be equivalent. A detailed list of topics was presented that serves to indicate the direction which changes should take to answer the need.

3. Can a one-hour laboratory be made worthwhile? John S. Rinehart, *Wayne University, Detroit, Mich.*—The curriculum at Wayne University provides only 1 hr/wk for the beginning liberal arts physics laboratory. It was felt inadvisable either to simplify the laboratory so that the student merely took readings or to reduce it to a lecture-demonstration period. Instead, the desire was to make the laboratory a place where the student (1) can see for himself that many everyday experiences, if properly analyzed, are legitimate physics experiments, (2) can have fun, (3) can develop some manipulative skills, (4) can try things for himself and (5) can test a few physical principles. Obviously no single experiment meets all these criterions, and hence our experiments differ widely from week to week, some fulfilling one objective, some another. The apparatus is made simple and directly applicable; for example, actual wheelbarrows, bridges, street lamps and pictures are used to illustrate levers and forces. Such difficult experiments as the Atwood's machine, when stripped of exacting manipulative skills, can be performed and understood. The student is given a choice of apparatus, is furnished with little detailed procedure and sometimes he may do something of his own choosing. The students seem to show a gratifying interest in the experiments and a surprising amount of initiative and curiosity.

4. Non-numerical physics for nonscience students. Clarence E. Bennett, *University of Maine, Orono, Me.*—Although the nonscience student needs and is interested in a limited amount of physics, it is not easy for him to obtain a serious yet unprofessional introduction to the subject in either the conventional course or the present-day survey course. In the belief that it is the numerical part of the subject which repels the student and arouses the familiar comment that he "never could see mathematics," the author has developed a non-numerical but otherwise serious and sober physics course that emphasizes concepts, definitions and technical vocabulary but omits practically all formulas and arithmetic. The amount of conventional elementary material that can be treated nonarithmetically is surprisingly large. Furthermore, the nonmathematical mind is not necessarily unable to distinguish between an appreciation of the science and a false sense of comprehension of it. Not competing in any way with other courses in a department which has recently developed a rather strong and successful engineering physics curriculum, this course attracts many students who would never otherwise come into direct contact with physics. Perhaps a course of this type should also be introduced into the secondary school, where it might help to solve the problem that exists there. Should the College Entrance Examination Board be urged to allow more emphasis to be placed on concepts and less on formulas in the secondary school course?

5. An integrated physics-mathematics course for the ESMDT program. Marsh W. White, *The Pennsylvania State College, State College, Pa.*—The departments of

physics and mathematics at the Pennsylvania State College are cooperating with the extension services of the college in giving, at some 100 defense training centers in Pennsylvania, an evening integrated course in certain parts of elementary mathematics and physics. About 3600 students in 155 classes are enrolled. The part time teachers are from nearby colleges, secondary schools, and industrial plants and laboratories. A supervisory and technical staff of 25 full and part time men plans the course and coordinates the instruction. The course is officially designated as "Foundations of Engineering." Its objectives are to upgrade employed defense workers and to provide pre-requisite material for more advanced technical training in science and engineering. Secondary school science and mathematics are not required. Although conventional individual students experiments are not performed, each period includes demonstration experiments in which both teacher and students participate. Numerous visual aids and exhibit apparatus are employed. Graphical methods, the slide rule, handbooks and simple measuring instruments are studied and used. The work in mathematics includes parts of arithmetic, algebra, geometry and trigonometry. The physics material is selected from the fields of mechanics, properties of materials, heat, electricity and magnetism.

6. (a) Teacher rating and (b) other findings in the Pennsylvania summer EDT program. C. J. Lapp, *State University of Iowa, Iowa City, Ia.*—(a) In rating the 112 individuals who taught college physics in the Pennsylvania summer EDT program, three methods were used: (1) a purely subjective rating given by the assistant supervisors who served as inspectors; (2) a semi-objective rating obtained from a questionnaire of 20 items answered by the assistant supervisor inspectors and afterwards evaluated objectively; (3) a purely objective method in which each student's achievement was evaluated in terms of his expected achievement and the average of these student ratings for each class was taken as the teacher rating. The ratings obtained by the three methods were, in most cases, in remarkable agreement. The degree of agreement between the three methods for any one district likewise gave a rating for the assistant supervisor in charge of that district.

(b) The aptitude testing program was probably the most elaborate ever used and gave excellent results. The *Cooperative physics test for college students*, Mechanics 1936 A, and a special objective test designed to measure ability to think critically were used as achievement measures. The distribution obtained by these measures has been compared with national norms. A study was also made of the effectiveness of cumulative review sheets in producing achievement.

7. The mobile demonstration laboratory of the Pennsylvania summer EDT program. Harold K. Schilling, *The Pennsylvania State College, State College, Pa.*—Since equipment suitable for lecture-demonstrations was virtually nonexistent in the teaching centers of the summer program of the Pennsylvania State College, a mobile demonstration laboratory was developed as a teaching aid for the electricity and magnetism portions of the course. The equip-

ment for a demonstration and "m" student participation by apparatus at two laboratory centers were a demonstration program be cont...
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ment of this laboratory was of two kinds: that designed for a demonstration lecture entitled "Electrons at work," and "museum exhibits" of the push-button type, which students could manipulate after the lecture. Transportation by truck and unique methods of mounting and packing apparatus made it possible to offer a lecture and exhibit at two different centers each day. The staff of this laboratory consisted of two lecturers and three assistants. In answers to a questionnaire the teachers participating in the program, as well as local administrative heads of centers, were almost unanimous in the opinion that this mobile demonstration laboratory was of great value and should be continued in future programs.

8. Demonstration experiments. Eric M. Rogers, *Saint Paul's School, Concord, N. H.*—(a) *Use of ground glass in demonstrating distortion in lens images.* In the usual demonstration, a lens forms a real image of a rectangular pattern, and a small stop placed (*i*) in front of and then (*ii*) behind the lens gives barrel and pincushion distortion, respectively. However, when the object is illuminated by a small source—an arc or a compact filament—with or without a condenser, it is not possible to obtain wide, well-illuminated images in both cases. To give such images, rays from each object-point must pass through the stop and therefore an *image of the source* must be formed at or near the stop, and this condition cannot be fulfilled for both stop positions simultaneously. One should either move the source or change the condenser when changing from (*i*) to (*ii*), so that an image of the source is formed on the stop; or else one should insert a sheet of ground glass between the condenser and object, so that each object-point receives a wide cone of rays and thus transmits rays to the stop, wherever it is. The ground glass wastes light but gives clear pictures. (b) *Resonance.* Water is added to a flask until the latter is in resonance with an organ pipe. A slanting sheet of paper is fixed with its lower edge just above the mouth of the flask, and a small paper animal is placed on this "hill." When the pipe is blown, or the same note is sung, the animal hurries down the hill. The demonstration is most effective when projected as a shadow on the wall. (c) *The resultant of uniform and circular magnetic fields.* This field of force—the "catapult effect" field—is often sketched incorrectly in books. The correct diagram is easy to construct geometrically and affords a good exercise in dealing with equipotentials. The equipotentials of the two component fields are drawn, these being a set of parallel straight lines and a set of equally spaced radii from the straight wire, respectively. Then the resultant equipotentials are constructed by addition, and their orthogonals are sketched to give the lines of force.

9. Projection of thin-film interference fringes. Thomas B. Brown, *George Washington University, Washington, D. C.*—The commercial high-pressure mercury-arc lamp (H4 or similar type) serves as a source of "monochromatic" light with which it is possible to demonstrate by projection many interference phenomena that otherwise could be exhibited to but one student at a time. One of the most interesting demonstrations is the ordinary test for optical

surfaces by means of an optical flat. However, success in projecting fringes produced by such a comparatively thick film requires more than a suitable source of light. Normal incidence is impractical for various reasons; and when oblique illumination is used, the fringes, which then appear at a considerable distance from the air film itself, are badly astigmatic unless the rays are essentially parallel. An angle of incidence of 45° has been found most suitable for the apparatus which has been developed for this demonstration; parallel rays are insured by placing a diaphragm at the second principal focus of the projecting lens, in addition to a condensing lens in front of the light source. The projecting lens has a wide range of focusing adjustment, since the fringes appear sometimes below the air film and sometimes above it. Best definition is obtained when the fringes are perpendicular to the plane of incidence. The apparatus is quickly and easily adjusted.

10. Dew point determination by means of a photoelectric cell, galvanometer and thermocouple. J. J. Coop, *Washington College, Chestertown, Md.*—In determinations of relative humidity by the dew point method, students often experience considerable difficulty in detecting the appearance and disappearance of the dew. While the use of the telescope permits observations at some distance from the surface, the same difficulty exists and also the added one of reading the thermometer. These difficulties have been largely overcome by using a photoelectric cell and galvanometer to detect the change in reflected light from the surface and a thermocouple and the same galvanometer to measure the temperature. A polished surface is cooled by forcing air through ether. Light from a 6-v lamp is reflected from the surface and focused on a Weston Photronic cell, which is in series with a portable galvanometer, Weston type 440. The instant that the dew appears on the surface, the galvanometer shows a deflection; as the dew disappears the needle returns to its initial position. The temperature at these two points is found by switching the galvanometer from the photoelectric cell to the thermocouple. The temperature is read from a calibration curve.

11. Visualization of the conjugate points of a lens. Ira M. Freeman, *Central College, Chicago, Ill.*—The familiar nomograph for finding pairs of conjugate foci for a thin converging lens is modified to exhibit both foci in their proper positions on a single line—the axis of the lens—and is extended to include the case of a diverging lens. The arrangement is constructed in the form of a simple mechanical model which gives a continuous and accurate representation of the location of the image as the object is moved relative to the lens. The slow approach of the image to the principal focus as the object recedes to infinity, the existence of a minimum object-image separation at the symmetric position, and other important features can thus be shown in a vivid way. A model of the device is demonstrated by projection.

12. A gyroscope experiment for the advanced dynamics laboratory. Paul F. Bartunek, *Rensselaer Polytechnic Institute, Troy, N. Y.*—An experiment has been developed to

test the relation, $mgh = I\omega\Omega$, for a gyroscope in steady precession, where mgh is the product of the weight which produces the precession and the distance measured along the axle from the center of mass to the point where the weight is suspended, I is the moment of inertia of the wheel, and ω and Ω are the angular speeds of spin and precession, respectively. A double pole, single throw switch is arranged to close the circuit of an electric timer and an impulse counter simultaneously. A contact maker mounted on the gyroscope closes the impulse counter circuit once each rotation; the number of rotations of the wheel are thus counted during the time of one or more rotations of the system in precession. From these data and with the aid of curves displaying ω and Ω as linear functions of the time—obtained by means of a stroboscope and a chronograph—instantaneous values of ω and Ω are obtained. An auxiliary experiment serves to give I . The product $I\omega\Omega$ is then compared with mgh . The experiment has been performed with three different gyroscopes; the agreement is within the limits of experimental error.

13. Semi-automatic mapping of two-dimensional fields. Milan W. Garrett, *Swarthmore College, Swarthmore, Pa.*—The conventional voltage divider and pantograph are used to trace equipotentials in a 36-in. electrolytic tray with circular plate glass bottom and a rim cut from an automobile inner-tube. Recording is automatic. When off balance, the 1000-cycle/sec input voltage is amplified and rectified, biasing negatively the grid of a small thyratron (885). At balance, the bias disappears and raw 60-cycle/sec current passes through the primary of a neon sign transformer in the thyratron plate circuit. Sparks from the secondary pass through waxed paper to a metal plate and record the position of the pantograph tracing arm. A 0.5-mv signal will control the spark, while speed is limited only by the necessity of staying within this narrow voltage limit for 1/60 sec. Since 30 v are applied to the tray, accuracy is limited at present by the pantograph design. For quantitative results, boundary conditions at the edge of the field must be carefully considered. Often it is easy to set up the conjugate of the problem in hand and so to trace quantitative flow lines directly on the same diagram. Examples of field maps were shown.

14. Some stepped-up lecture table experiments. Richard M. Sutton, *Haverford College and University of Minnesota*.—Five modifications of simple experiments were shown: (1) a new disk with movable center of gravity that will either rotate about its center of figure or wobble when tossed into the air; a shifting weight enables the operator to throw the disk with either motion at will; (2) a simplified method of demonstrating Newton's third law by use of a plank and two laboratory rods instead of an expensive car or cart; (3) a "falling body" whose moment of inertia is such as to give it a vertical linear acceleration of only 1 cm/sec² when it is allowed to fall under gravity; (4) an improved method of illustrating atmospheric pressure and vapor pressure by making a 1-l flask "drink" 800 ml of water from a beaker situated 1 m below it; (5) a disarm-

ingly simple mechanical conundrum that catches the unwary; it involves only two spring balances, a light wooden frame and a single 1-kg weight.

15. A luminous bridge. W. B. Pietenpol, *University of Colorado, Boulder, Colo.*—A device has been constructed for demonstrating to large groups, by means of incandescent lamps, the current that passes through various parts of a bridge circuit. The apparatus may be operated with either a.c. or d.c., commercial line voltage. An interchange of lamps of various powers gives a balanced or unbalanced arrangement. A small, low-voltage lamp is used to indicate the condition of balance. A tubular rheostat with sliding contact may be used in place of one of the parallel lamp circuits; the condition of balance with varying contact is shown by the indicator lamp. The device may also be used for demonstrations of resistances in series and parallel, the resistances of lamps of various powers, and the negative temperature coefficient of a carbon filament.

16. Efficiency of instruction in college physics. Charles W. Edwards, *Duke University, Durham, N. C.*—A study was made of the relation of cost of input to value of output of the process of instruction in general physics courses. Various types of instructional effort were studied. For instance, the effectiveness of an elaborate lecture-demonstration or of the careful derivation of a formula by the lecturer was determined by means of immediate "pop" tests. It was considered of especial interest to determine the percentage of a large group that could indicate either the principle involved or the technic employed in a laboratory experiment two weeks after it was performed. Tests were administered to medical and engineering students to determine their "carry over" after entering professional schools. The results of the whole study indicate rather clearly that a considerable waste of time and effort is common. Much of the material that is laboriously and painstakingly presented by the instructor never "gets across" to the student. Much more permanent and valuable results probably would follow if the material presented in one year were severely limited.

Annual Report of the Treasurer

Balance brought forward from Dec. 16, 1940	\$3006.40
CASH RECEIVED	
Dues received ¹ for 1941	\$4352.50
Dues received for 1940	35.00
Dues received for 1942	212.50
Grant	1500.00
Royalties, <i>Demonstration experiments in physics</i>	306.79
Membership fee for A.C.E., paid by American Institute of Physics	100.00
Donations	28.85
<i>Total deposited, 12/16/40 to 12/15/41.</i>	6535.64
Total cash available	\$9542.04

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DISBURSEMENTS

Postage and supplies.....	\$ 197.66
Printing.....	48.62
Stenographer, Editor's office.....	609.00
Secretary's office expense.....	382.50
Constituent membership in A.C.E.....	100.00
Editor's traveling expense.....	35.95
Payments to American Institute of Physics.....	3325.03
Traveling expense of representatives of A.C.E.....	74.40
Discount on Canadian and Hawaiian checks.....	1.09
Journal survey articles.....	18.50
Money advanced on <i>Demonstration experiments in physics</i>	306.79

Biographical dictionary for Editor's office.....	7.00
Certificate for Oersted Medal.....	8.75
<i>Total disbursed</i>	5115.29
Balance on hand ¹ Dec. 15, 1941.....	\$4426.75

PAUL E. KLOPSTEG, *Treasurer*

I have audited the books of account and records of Dr. Paul E. Klopsteg, Treasurer of the American Association of Physics Teachers, for the year ending December 15, 1941, and hereby certify that the foregoing statement of receipts and disbursements correctly reflects the information contained in the books of account. Receipts during the year were satisfactorily reconciled with deposits as shown on the bank statements, and all disbursements have been satisfactorily supported by vouchers or other documentary evidence.

Chicago, Illinois,

December 22, 1941.

WILLIAM J. LUBY
Certified Public Accountant

¹ On December 16, 1941, there were 909 members in good standing.
² A balance of approximately \$600 is due the American Institute of Physics for the publication of the journal in 1941.

THE DALLAS MEETING, DECEMBER 29-30, 1941

A REGIONAL meeting of the American Association of Physics Teachers was held jointly with Section B, American Association for the Advancement of Science, at Dallas, Texas, on December 29-30, 1941. The program for the meeting was arranged by Professors C. W. Heaps, F. M. Durbin, William Schriever, Newton Gaines, L. B. Ham and Frank C. McDonald, *Chairman*.

Invited Papers

Three invited papers were presented on Monday afternoon, December 29, in Hyer Hall of Physics, Southern Methodist University:

Applications of electron scattering. A. L. Hughes, *Washington University*, Retiring Vice President of Section B.

Stereo-microscopy with the electron microscope. V. K. Zworykin and J. Hillier, *RCA Manufacturing Company, Incorporated*.

Electron micrograph studies of insect structures. A. Glenn Richards, Jr., *University of Pennsylvania*, and Thomas F. Anderson, *RCA Fellow of the National Research Council*.

The following papers on various aspects of applied geophysics were heard in two sessions held in the auditorium of the Dallas Power and Light Company, on Tuesday, December 30:

Seismology. Cecil H. Green, *Geophysical Service, Incorporated*.

Electrical methods in prospecting. Dart Wantland, *Colorado School of Mines*.

The gravimeter. D. H. Clewell, *Magnolia Petroleum Company*.

The theory of hydrocarbon reactions. Everett Gorin, *Magnolia Petroleum Company*.

Applications of spectroscopy to the oil industry. J. Rud Nielsen, *University of Oklahoma*.

Geophysical exploration and its part in national defense. C. A. Heiland, *Colorado School of Mines*.

Training of physicists for work in geophysics. J. C. Karcher, *Coronado Company*.

Training of students for work in the petroleum industry. E. A. Stephenson, *University of Kansas*.

Contributed Papers, with Abstracts

The morning session on Monday, December 29, was devoted to ten contributed papers. The abstracts for papers No. 1 to 5, which pertain to the interests of Section B, will be found in the February 1-15, 1942 issue of the *Physical Review*.

1. Mechanical means for the graphical representation and solution of transcendental functions. Lisle L. Wheeler and S. Leroy Brown, *University of Texas, Austin, Tex.*

2. Vibration rotation energies of planar XYZ₂-molecules. Samuel Silver, *University of Oklahoma, Norman, Okla.*

3. Raman spectra of compounds in the gaseous and liquid states. Newton D. Ward, *Magnolia Petroleum Company*, and J. Rud Nielsen, *University of Oklahoma, Norman, Okla.*

4. Vibrational analysis of the 3200A band system of carbon disulfide. Gene T. Pelsor, *University of New Mexico, Albuquerque, New Mex.*

5. The parabolic and logarithmic oxidation of copper. H. A. Miley, *Oklahoma Agricultural and Mechanical College, Stillwater, Okla.*

6. Production of electric charges in water spray. C. W. Heaps, *Rice Institute, Houston, Tex.*—The Simpson theory of charge production in thunderstorms is based upon the fact that air currents, in breaking up water drops, produce ions. This phenomenon can be demonstrated to a class in a simple way. A gold leaf electroscope is constructed in a cubical glass cell of edges about 4 cm. The slender gold leaf is stuck to the flattened end of a wire about 12 cm long. The other end of the wire extends outside the box and is bent downward along the axis of a piece of 1/4-in. copper tubing. A glass sprayer of the atomizer type, operated by compressed air, sprays air and water droplets through the copper tube. The presence of ions in the spray is proved by the falling of the leaf of the charged electroscope. With ordinary tap water and a good sprayer the leaf falls in about 4 sec. The whole apparatus, except the sprayer, is small enough to be projected on a screen. Lucite insulation is excellent in damp surroundings; however, the exhaust spray is directed away from the insulating block of the electroscope. It is well to show the class that the air jet alone will not discharge the electroscope.

7. Units in mechanics. E. A. Schuchard, *Amarillo College, Amarillo, Tex.*—The major difficulty experienced by engineering students in their beginning operations with mechanics units arises fundamentally in the failure of most current physics textbooks to adopt a consistent distinction between the gravitational units of force (gram force, pound force) and the corresponding units of mass (gram mass, pound mass). Hence it is proposed that the units of mass be consistently labeled, for example, as *gram-mass* (abbreviation, gmm) and *pound-mass* (lmm), while the names of the gravitational units of force be left as the *gram* (gm) and the *pound* (lb) to conform with their widespread usage in statics, in work units and in engineering applications. With some such distinction consistently made between the unit names of the logically distinct concepts of force and mass, the following desirable features can be shown to hold: (1) units can be checked without any possibility of contradiction; (2) a set of equivalence-relations between the cgs or fps system of units and the gravitational (or engineering) system can be set up; (3) by means of (2), problems with mixed units can be handled in a most straightforward manner; (4) by means of (2), any formula of mechanics can be simply changed from its physical to its engineering form, or vice versa; (5) by means of (4) the appearance or nonappearance of a g-factor is made clear; (6) by means of (2) and (4) the practical distinction between the two systems of units disappears, enabling engineering students better to apply and correlate their physics with their engineering courses.

8. Up-to-date experiments for the laboratory. Louis R. Weber, *Colorado State College of Agriculture and Mechanical Arts, Fort Collins, Colo.*—Although emphasis in the world at large is being placed on the acoustic treatment of rooms, sound intensity, proper illumination, air resistance to moving objects, radio transmission and reception, radioactivity and spectral analysis, practically no experiments, even of a simple nature, are given in general physics

laboratories to acquaint students with these fields. One reason has been the apparent belief that the apparatus is prohibitively expensive. However, some of the experiments involve no additional equipment, and others can be performed with apparatus easily made or purchasable at small expense. Some of the experiments and the equipment needed are: sound intensity levels (pocket watch); reverberation period (meter stick); speech intelligibility ratings of rooms (word lists); sound absorption coefficients (microphone and amplifier); illumination survey (shop-made extinction meter); absorption of α - and β -particles (electroscope and radioactive source); drag (simple wind tunnel, electric fan); spectral analysis of metals (grating and spark coil); vacuum tube oscillator (simple radio parts).

9. The mks system, its justification and use. R. E. Beam, *Southern Methodist University, Dallas, Tex.*—Three important actions towards standardization and simplification of mathematical symbolism in scientific writing were mentioned; all three are connected with systems of units and dimensions, especially with the mks system. To justify the adoption of the mks system of units and dimensions, the operational method of approach to the formation of unit and dimensional systems was stated and then applied to the case of the esu and emu systems in order to illustrate points of confusion to students who use both systems. The "rationalization" and "subrationalization" of systems of units and dimensions were discussed. The "absolute" practical system of units and dimensions using the meter, kilogram and second, together with some fourth magnetic quantity, following Giorgi, was then discussed and compared with other systems. A method of transposing textbooks based upon the emu and esu systems to the mks system was given.

10. Aviation problems in elementary physics. Earl W. Thomson, *United States Naval Academy, Annapolis, Md.*—The tremendous expansion in the construction and operation of airplanes offers an excellent opportunity for the teacher of elementary physics to introduce problems on aviation into the course and thus to modernize his instruction in the application of physics. Sample problems which may be solved are on the vector polygon of forces of an airplane in a climb or a dive, the laws of equilibrium applied about the three axes of a plane in the maintenance of stability, centrifugal force in banking and coming out of a dive, required and available power, efficiency of engine and propeller, and the Bernoulli theorem applied to sustentation. Sample problems were presented and solved.

Attendance at the Dallas Meeting

The registration of those in attendance at the Dallas meeting lists 38 members of the Association and 91 non-members. Members who registered were:

D. L. Barr, Welch Manufacturing Company; E. S. Barr, Tulane University; V. E. Bottom, Friends University; H. L. Dodge, University of Oklahoma; F. G. Fournet, Louisiana State Normal College; N. Gaines, Texas Christian University; R. C. Gibbs, Cornell University; N. S. Gingrich, University of Missouri; L. B. Ham, University of Arkansas; E. Haslewood, Texas Technological College; C. W. Heaps,

Rice Institute; A. L. Hughes, Washington University; Lilian Keith, Senior High School, Huntsville, Texas; C. T. Knipp, University of Illinois; F. E. Lowance, Centenary College; F. C. McDonald, Southern Methodist University; H. A. Miley, Oklahoma A and M College; J. R. Nielsen, University of Oklahoma; G. T. Pelsor, University of New Mexico; H. C. Roys, University of Oklahoma; R. A. Sawyer, University of Michigan; E. A. Suchard, Amarillo College; J. E. Shrader, Drexel

Institute; W. Schriever, University of Oklahoma; S. Silver, University of Oklahoma; O. W. Silvey, A and M College of Texas; M. N. States, Central Scientific Company; G. A. Stinchcomb, Heidelberg College; L. R. Weber, Colorado State A and M College; C. A. Whitmer, University of Oklahoma; C. S. Woodward, Central State College of Oklahoma; Charlotte Zimmerschied, Southern Illinois Normal University.

The Challenge—A Message from President Knowlton

NEVER, as I truly believe, has any group of teachers been called to attention by a sterner challenge than the members of the American Association of Physics Teachers at this time. President Conant of Harvard has well said that this is a physicists' war. And there is every reason to fear that it may be a long war. Some three of every four physicists of the country are directly engaged in defense work, and every day there are calls for more. The demand made upon those of us who are left in the classroom is that we shall not allow the stream of able and well-trained young men who must be added to this group in any twelve-months period to fail. We must recruit and train; labor as never before, and with more critical intelligence. Somehow we must attract more of the ablest students, give them better training in less time. How can it be done? Not, certainly, by any mysterious process,

but by adding a little here and a little there; by giving of ourselves more freely; by giving those students who are able to do so, an opportunity to progress more rapidly and to take on the responsibility of educating themselves rather than being educated.

Short courses and special training in some lines—radio, aviation, and so forth—will be asked of us, but our most important contributions will be in the trickles of qualified workers who go from our laboratories. How teaching efficiency may be improved is something that each of us must work out for himself. I venture to suggest that when the emergency is over, a recapitulation of the ways in which this end of intensified training has been accomplished, may constitute a valuable contribution to what we, as teachers, need to know.

Harry Sloan Hower, 1877-1941

PROFESSOR HARRY SLOAN HOWER, head of the department of physics at the Carnegie Institute of Technology, died on October 10, 1941 at Pittsburgh after an illness of three weeks.

Professor Hower was born at Parker, Pennsylvania, in 1877. His undergraduate work was done at the Case School of Applied Science, from which institution he held the M. S. degree. He also studied at the University of Berlin. After serving as instructor under the late Professor Dayton C. Miller at Case, he came to the Carnegie Institute in 1906, becoming head of the department in 1915. His brilliant lectures in general physics were an important contribution to engineering education at the Institute.

He founded the research laboratory at the Macbeth-Evans Glass Works at Charleroi, Pennsylvania, in 1919, and continued as its head until the firm was amalgamated with the Corning Glass Works in 1936. Afterwards he was a consultant for the latter company. He served many times as patent expert in litigation concerning glassmaking and

other processes. He was the designer of pressed lenses for the Panama Canal, and for the Army, the Navy and the Norwegian Merchant Marine. In the midst of these activities, Professor Hower found time to perform his duties as head of a college department. On this work his insight, enthusiasm and sympathy went far beyond routine. His wide experience and sound judgment were always at the disposal of students, alumni and faculty.

He was a fellow of the American Physical Society and of the American Association for the Advancement of Science; also a member of the American Optical Society, Society for the Promotion of Engineering Education, American Ceramic Society, Illuminating Engineering Society, English Society of Glass Technology, American Association of Physics Teachers, Pennsylvania Academy of Science, Sigma Xi, Tau Beta Pi and Theta Xi.

Survivors are his widow, Sara Chester Hower, three sons, a daughter and a grandson.

CHAS. WILLIAMSON

Report of the Committee on the Teaching of Physics in Secondary Schools

At the ninth annual meeting of the American Association of Physics Teachers, in Columbus, December, 1939, a committee was appointed to study the teaching of secondary school physics and to bring about a better coordination of the natural sciences and mathematics in the schools. The members of the committee are: H. W. LeSourd, Milton Academy; R. J. Stephenson, University of Chicago; G. W. Warner, Wilson Junior College; and K. Lark-Horovitz, Purdue University, *Chairman*. At the tenth annual meeting in Philadelphia, December 1940, the work of this committee was approved by the Executive Committee of the Association, and the committee was continued for another year with the addition of two new members: R. C. Gibbs, Cornell University, and J. R. Oppenheimer, University of California.

After making a study of available reports on the high school physics situation, the committee felt that information should be obtained concerning the other science fields and accordingly contacted officers of the American Chemical Society, the American Biological Society, the Mathematical Association and other similar organizations.

A tentative plan was worked out to consider first the facts available, then to recommend a definite change in teacher training requirements that would stress subject matter material, to recommend a reduction in the number of hours required in professional education, to establish a natural grouping of licensing, to agree on a plan of minimum essentials for the high school curriculum and a plan to stress the cultural and historical background of the sciences and their interrelations, and to review the textbooks available, with recommendations for mastery of certain standard material.

The American Chemical Society made a study of the high school teaching of chemistry and the report of the committee has been published.¹ The American Mathematical Association appointed a commission "On the Place of Mathematics in Secondary Schools." Working over a period of several years, this commission has studied in detail the mathematical curriculum and also the education of teachers for the high school. Its report is available in book form. The Union of Biological Societies has made a careful study during the last year and this study is available.² Detailed studies have been made for the teaching of physics in high schools in the state of Pennsylvania, by M. H. Trytten and James M. Leach,³ and in the state of Arizona, by E. H. Warner.⁴ Other studies have been made for the states of Indiana, Illinois, Minnesota and Iowa. Most of these results have not been published. The National Science Teaching Committee has a subcommittee on teacher training which has made detailed recommendation regarding the training of science teachers. This committee, under the chairmanship of S. R. Powers, Teachers College, Columbia University, recommends a strong subject matter training but with definite stress on the social and community application of the natural sciences.

Study of the data obtained from all fields of science

showed common defects in the preparation of teachers, in the existing regulations for certifications of teachers, in subject matter assignment of teachers and in many other practices that are detrimental to the promotion of education in the basic sciences.

In April, 1940, at Washington, the committee obtained permission from the Executive Committee of the Association to approach the other scientific societies officially in order to establish a cooperative plan for correlating the work of the subject matter departments and of the departments of education. It was decided that an attempt should be made to arrange a meeting of representatives of the various societies at the time of the A.A.S. meeting in Philadelphia, or at an earlier date if possible. These contacts were made during the summer and fall. A preliminary group meeting, in charge of R. M. Sutton, was held at Cleveland, on November 23, 1940; K. Lark-Horovitz reviewed the work of the committee and told of work done in Indiana by a cooperating group which has led to recommendations to the Indiana State Board of Education.

The joint science meeting, in charge of K. Lark-Horovitz, was held at Philadelphia as planned, with representatives from organizations in chemistry, biology, mathematics and physics attending. As a result of the discussion, an all-science Cooperative Committee on Science Teaching is now in the process of formation. At a meeting for organization held on April 19, 1941, R. J. Havighurst, University of Chicago, was elected chairman of this cooperative Committee.

Since no funds were available for any extensive investigation it was necessary to limit the actual work of the Association committee to problems that could be studied locally. R. J. Stephenson and G. W. Warner, members of a committee of the Central Association of Science and Mathematics Teachers, obtained five scholarships for a workshop experiment during a five-week period at the University of Chicago.⁵ K. Lark-Horovitz, as chairman of the Indiana Physics Teachers Training Committee, has considered in particular the problem of teacher training. The present curriculum leading to a license in any of the natural sciences was reconsidered at Purdue University in joint meetings of the departments of biology, chemistry, mathematics and physics with members of the department of education. It was agreed that only 12 semester-hours of professional education should be required of teachers, that the teaching of methods should be administered in the subject matter department, and that any advanced degree should be taken with a thesis under the joint supervision of the subject matter and education departments in the respective subject matter laboratory. It has been proposed that, in addition to the fundamental courses in subject matter, the prospective teacher shall take survey courses in biology, chemistry, physics and mathematics which will enable him to understand the historical and cultural background of the sciences. The student-teacher supposedly would help in the preparation of the material for one of these courses, would study the performance of

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the University instructor administering the course and would help to collect the literature necessary for the studies of the historical development. By preparing mimeographed write-ups for each week, the student-teacher will have appropriate classroom material already prepared to draw upon in his future performances in the high school. Survey courses as part of the teachers training are now also proposed at the University of Chicago.

The problem of the professional improvement and in-service training of high-school teachers has been considered separately. Special training courses with laboratory and lecture demonstrations have been provided in the summer session at Purdue University. It is proposed to establish short courses throughout the year to make it possible for the high school teacher to discuss his problems with the college teachers responsible for teachers training.

We propose that departments of education should canvass the need for visiting lecturers in the different high schools. By circularizing the subject matter departments in its institution, the education department could determine the number of staff members available as visiting lecturers and the subjects of the lectures. On the basis of previews of the proposed lectures it would be possible to supply the high schools with an approved list of available lecturers.

We recommend correspondence courses for teachers who are in small schools remote from the university or unable to afford visiting lectures.

It is the opinion of this committee that our aims can be reached only in full cooperation with the various departments of education. This is necessary, for example, for the construction of tests and examinations that will truly measure the achievement of the student and that can be used by education departments in giving the students proper vocational guidance.

A logical presentation of the foundation of science is necessary for those high school students who receive their last schooling. An insight into the ideology of the sciences and mathematics is essential to an appreciation of their esthetic values and to an acquaintance with the close correlation of the various branches of scientific effort. We feel that the present high school science courses which are intended primarily for college preparation are unsuitable for the great majority who never go to college. We also feel that the superficial treatment in a so-called general science course or in a course that stresses applications only, does not lead to a real understanding of scientific methods and of fundamental principles. We believe that a revision of the science curriculum in the high school must be made in such a way that minimum essentials are established for all the sciences, including mathematics. These minimum essentials will be discussed by the joint committee representing the various sciences in collaboration with departments of education.

K. LARK-HOROVITZ, Chairman

¹ Ind. and Eng. Chem., **14**, 147 (1936).

² Am. Bio. Teacher, April 1, 1941.

³ Am. J. Phys. **9**, 96 (1941).

⁴ Am. J. Phys. **9**, 368 (1941).

⁵ For a report on the work of this group, see Am. J. Phys. **9**, 50 (1941) or Sch. Sci. and Math. **41**, 548 (1941).

DIGEST OF PERIODICAL LITERATURE

APPARATUS AND DEMONSTRATIONS

Small-scale electrical experiments. C. W. HANSEL, *Sch. Sci. Rev.* **23**, 47-60 (1941). A watch glass may be used as a container for electrolytes in many experiments; the electrodes are short lengths of wire supported so that their ends are a few millimeters apart in the liquid; a magnifying glass or a low-power microscope facilitates observation. A glass U-tube with limbs 4 or 5 in. long can be used as a simple voltaic cell for study of polarization, emf and resistance, for measuring the conductivity of electrolytes, for electrolysis of various solutions, as a secondary cell, and as a chemical rectifier; it will serve as a Daniell cell if the copper sulfate and zinc sulfate solutions are separated by a plug of cotton or of glass wool in the bend of the U-tube.—J. D. E.

Soluble anhydrite: a universal desiccant. G. FOWLES, *Sch. Sci. Rev.* **23**, 20-22 (1941). Anhydrous calcium sulfate exists in two crystalline forms, one of which is called *soluble anhydrite*. For some time it has been known as a good desiccant. It can be prepared from scraps of blackboard chalk, some kinds of which consist of hydrated calcium sulfate (gypsum). The corners of a square of wire gauze are bent down so that the gauze can be supported about 0.5 in. above the bottom of a tin can. The can is filled with pieces of chalk and is covered; a thermometer is inserted through a hole in the cover. The can is heated over a low Bunsen flame to a temperature of 230°-250° for 1-2 hr. The soluble anhydrite formed in this way can be stored in small glass bottles. It will instantly absorb 6.6 percent of its own weight of water, does not become wet or crumble, is not corrosive and does not react with most inorganic liquids. Thus it is a good desiccant for use in balance cases, or for drying alcohol and other liquids. It will even dehydrate concentrated sulfuric acid.—J. D. E.

Inexpensive thermionic voltmeter. T. B. RYMER; *J. Sci. Inst.* **18**, 166 (1941). Most laboratories are well provided with inexpensive, robust galvanometers capable of detecting a current of 1 μ A or less, but, because of their short scale, incapable of measuring it accurately. The slide-back type of peak voltmeter utilizes such a galvanometer but, as usually described, requires a d.c. voltmeter as well. However, the present note shows that this voltmeter may be replaced by a potentiometer, such as an ordinary radio volume control; thus, galvanometers of the type which are most abundant can be converted with small cost and labor into thermionic voltmeters of sufficient accuracy for most purposes. In Fig. 1, the a.c. voltage to be measured is applied to the terminals *A* and *B* and charges the condenser *C* through the diode rectifier. The potentiometer *P* is adjusted until the galvanometer *G* reads zero. Since the setting of *P* needed to give this balance depends on the voltage *AB*, the pointer of the knob controlling *P* may be arranged to move over a scale graduated to read directly

the rms value of AB on the assumption that this is sinusoidal. The precision attainable depends on the sensitivity of the galvanometer; if this can detect $0.5 \mu\text{A}$, the reading of P will be correct to about 0.03 v , rms. To this degree of accuracy, the calibration is found to be unchanged by

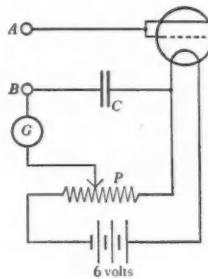


FIG. 1. Inexpensive thermionic voltmeter.

voltage variations of the 6-v storage battery, provided the latter is in reasonably good condition. The values of the circuit constants are not critical. In the author's apparatus, a 2-v detector tube with grid and anode connected together is used for the diode (a small 2-v power tube is equally satisfactory); C is $0.1 \mu\text{F}$; P is a 50-ohm radio potentiometer (or a 50-ohm resistor shunted by a 10⁴-ohm potentiometer). The range of his instrument is 0.5–3 v. Since the galvanometer current is zero if the reading of P equals or exceeds the voltage AB , with a consequent possibility of error due to this ambiguity, it is preferable to adjust P to give a small predetermined current in G , rather than exactly zero current. To do this, set the zero-adjuster of G so that, when no charge flows, the pointer is about $\frac{1}{4}$ div off the zero mark; then place the pointer of P on the zero mark, and calibrate the scale of P accordingly.—D. R.

CHECK LIST OF PERIODICAL LITERATURE

Some mechanics' own tools. R. S. Clay; *J. Sci. Inst.* **18**, 109–113 (1941). Among the home-made tools described are two holders to support thin sheet metal while it is being filed, a "wobbler" for exact location of a piece of metal, bent screwdrivers, punches for special purposes, a spotting tool, and so forth.

Some recent developments in meteorological instruments. F. J. Scrase; *J. Sci. Inst.* **18**, 119–125 (1941). Most of the recent developments are not so much radical changes in design as the adoption of existing designs to give remote indication or remote recording.

Karl Pearson: founder of the science of statistics. S. S. Wilks; *Sci. Mo.* **53**, 249–253 (1941).

Christopher Wren, F.R.S. D. Stimson; *Sci. Mo.* **53**, 360–367 (1941). "Possibly the greatest Englishman after Shakespeare."

The myth of poison gas. M. Goran; *Sci. Mo.* **53**, 374–376 (1941). In the last war the poison gas toll was less than 1 percent of the total toll, with the great majority of casualties occurring in the years before gas masks had been developed.

Static electricity and automobiles. R. Beach; *Sci. Mo.* **53**, 389–392 (1941). The "drag chain," commonly seen dangling from the rear of gasoline tank trucks, and conductive rubber tires are equally ineffective for grounding a vehicle; the resistivity of dry roadway material is too large.

Inventions and war. Q. Wright; *Sci. Mo.* **53**, 526–541 (1941). The development, characteristics and political effects of modern military technic as influenced by invention.

A skeptic among the scientists. R. Suter; *Sci. Mo.* **53**, 565–568 (1941). David Hume, "that rare thing, a skeptic among the scientists."

Mathematics of engineering. E. R. Hedrick; *J. Eng. Ed.* **32**, 57–66 (1941). Emphasizes "the need for action to offset the tendency toward degeneration in the secondary school" and the need for certain reforms in college teaching.

Physics in the engineering curriculum. E. U. Condon; *J. Eng. Ed.* **32**, 67–71 (1941). On "the deplorable lack of close association that exists between physicists and engineers."

Principles of flight. J. B. Marriott; *Sch. Sci. Rev.* **23** 27–35 (1941). A brief and elementary discussion of the forces supporting an airplane and how they are modified by wing shape, flaps, and so forth.

Some experiments on rotation in viscous liquids. A. D. Bulman; *Sch. Sci. Rev.* **23**, 36–46 (1941). A description and discussion of several experiments on fluid friction; a disk immersed in the liquid is rotated by a clockwork motor.

Molecular models with free rotation. C. E. Black, M. Dole; *J. Chem. Ed.* **18**, 424–427 (1941). Describes a new type of molecular model in which the wooden spheres representing atoms are securely joined by snap fasteners that permit internal rotation of the model.

A demonstration of adsorption. G. W. Smith; *J. Chem. Ed.* **18**, 432–433 (1941).

The electron-diffraction method of determining the structure of gas molecules. R. Spurr, L. Pauling; *J. Chem. Ed.* **18**, 458–465 (1941). A review article.

Benjamin Franklin and aeronautics. I. B. Cohen; *J. Frank. Inst.* **232**, 101–128 (1941).

Benjamin Franklin as a scientist. R. A. Millikan; *J. Frank. Inst.* **232**, 407–423 (1941). The author lists Franklin as one of the 14 most influential scientists of the 15th to the 19th centuries.

"Meet Doctor Franklin." C. Van Doren; *J. Frank. Inst.* **232**, 509–518 (1941). "Franklin had the most eminent mind that has ever existed in America."

The scattering of light in crystals. C. V. Raman; *J. Frank. Inst.* **232**, 203–211 (1941). A general discussion of the fundamental question, *why does a crystal scatter light?*

The radiosonde: the stratosphere laboratory. E. T. Clarke, S. A. Korff; *J. Frank. Inst.* **232**, 217–238, 339–355 (1941). A review of the most recently developed tool for the investigation of the upper atmosphere.

The teaching of the scientific attitude by means of selected topics in physics. I. Auerbach, A. Bader, J. L. Bassiches, M. M. Offner, A. Taffel; *Sch. Sci. and Math.* **41**, 740–746 (1941). A summary of a committee report on school physics made to the Physics Club of New York.